THE AIM AND ACHIEVEMENTS

OF

SCIENTIFIC METHOD:

AN EPISTEMOLOGICAL ESSAY.

BY

T. PERCY NUNN, M.A., D.Sc.

VICE-PRINCIPAL OF THE LONDON DAY TRAINING COLLEGE.

London:

MACMILLAN AND CO., LIMITED,

1907.

LONDON:
HARRISON AND SONS, ST. MARTIN'S LANE, W.C.,
PRINTERS IN ORDINARY TO HIS MAJESTY.

PREFACE.

THE following essay is an expansion of a paper read before the Aristotelian Society in February, 1906. I have to acknowledge the kindness of the Committee, which has permitted me not only to embody here most of my original matter, but also to make use of the type in which it was standing. The work in its present form (with the exception of one or two trifling emendations) was printed in September, 1906, and presented to the University of London as a thesis for the degree of Doctor of Science.

The results described in the essay were reached in the course of a study of the problems of Science teaching in schools, and are believed to have very definite pedagogical applications. These applications are not considered in the present volume, but, in view of the paucity of writings in English which treat of the pedagogy of Science upon a philosophical basis, I have thought it legitimate to direct the attention of the interested reader to the places where I have discussed some of my topics from the professional point of view. For the same reason I venture to refer to the chapter on Science Teaching in Professor J. W. Adamson's book on *The Practice of Instruction*.

The customary acknowledgements that have been made whenever I have appealed consciously to the work of other writers should be supplemented in two particulars. Even my numerous footnote references to the *Principles of Mathematics* hardly suggest the full amount of the inspiration which I have drawn from the work of the Hon. Bertrand Russell. I am

conscious of the influence of his mode of thought throughout the book. The fact that I have mentioned Professor Karl Pearson only in connexion with expressions of dissent from his views makes me wish the more to own that the day on which I looked into *The Grammar of Science* was the day when I first surveyed as from a "peak in Darien" the attractive waters which I have sought a little further to explore.

May, 1907.

CONTENTS.

CHAPTER I.

CHAPTER II.

CHAPTER III.

CHAPTER IV.

CHAPTER V.

THE AIM AND ACHIEVEMENTS OF SCIENTIFIC METHOD.

CHAPTER I.

§ 1.

THE words "Science" and "Philosophy" seem not unhappy examples of a distinct class of terms—terms that are recognised everywhere as indispensable and yet can find few who will accept the responsibility of giving them precise and exhaustive definitions. The phrase "Science and Philosophy" is felt probably by most educated persons to express a certain antithesis; but the vague and unanalysed character of this antithesis is shown (for example) by the "oddly alternative way" in which the two words are used in such names as "Mental Philosophy" and "Mental Science."*

The causes of this confusion are mainly historical. The modern spirit of inquiry in its first youth addressed itself naively and indifferently to all the problems which the world presented to it, and only learnt gradually to distinguish essential differences of character between the various questions which it raised and to elaborate distinct methods of dealing with them. In England, in particular, where speculation has rarely wandered far from the needs of practice, the antithesis alluded to above was, until some years into the nineteenth century, recognised only confusedly and imperfectly in the distinction between "moral" and "natural philosophy." The

* Sidgwick, *Philosophy: its Scope and Relations*, London, 1902, p. 2.

comparative independence and isolation in which the great English scientists of the eighteenth century worked tended to keep them to the universal point of view which seems always to have been an important part of the content of the notion of "philosophy," and hindered the division of labour which ends in the development of the well differentiated lines of inquiry and interpretation to which we apply nowadays the plural, "sciences." Thus we may say (in the opinion of Mr. Merz)* that the word "Science" did not reach its present meaning among us until about the time of the foundation of the British Association (1831) and that its displacement of the older "philosophy" was due partly to the fact that this term was beginning to bear in England the special significance which it had gained through the work of the great German metaphysicians; and partly to the dominant influence of France in all the "sciences." In France the co-operation of investigators under the patronage and with the assistance of the State had led to just those results for which England during the eighteenth century lacked the conditions, and the "Academie des Sciences" had, practically since 1666, used the word in its present familiar sense. We may conclude, perhaps, that only since about 1830 has European thought been fully conscious of the existence of the antithesis to which the opening paragraph drew attention.

On account, doubtless, of this imperfect apprehension of the distinction between Science and Philosophy the adjectives "scientific" and "philosophical" have followed in the development of their application courses different from those of the corresponding nouns. Thus, while there can be no doubt that the word "Science" refers most commonly to a certain body of knowledge, "scientific" refers more frequently to the method or procedure by which this knowledge has been established. By

---

* *History of European Thought in the Nineteenth Century*, 1904, i, p. 89.

an easy extension of its meaning the adjective is applied to *all* processes by which knowledge is reached having the security that is thought to characterise "Science." Hence "scientific" comes to be wider in its application than "Science"—to have in fact much the same range and implication as the German "Wissenschaft" which, as Merz points out,* has acquired a moral as well as an intellectual significance, and implies an ideal embracing at once the highest aims of the "exact" (*i.e.* "scientific" in the narrower sense precisely correlative to "Science"), the historical, and the philosophical lines of thought. To distinguish it from this use of "scientific" as implying an ideal of *exact* knowledge of things as they really are, free from conjecture and subjective distortion, we may perhaps assert that the typical use of "philosophical" is to imply a certain *breadth* of knowledge and of view, an ability to deal with new problems by the help of considerations drawn from a large mass of well-ordered experience and instructed thought. Thus, while, according to modern usage, the substantives "Science" and "Philosophy" undoubtedly refer to different provinces on the map of intellectual effort, however difficult it may be to delimitate their frontiers, the corresponding adjectives are, in general, not so restricted in their application, but are commonly used to characterise aspects of intellectual effort wherever it is exercised. Nevertheless they have *special* senses in which they correspond precisely to their nouns, and it should be understood that it is in this narrower sense precisely correlative to the noun "Science" that the term "scientific" is used in these pages—a sense which it will be necessary to determine with some care. The first and second of the following chapters will be devoted to this task of determining the aims of Science, while in the remaining chapters a critical estimate of the achievements of Science will be attempted.

---

* *Op. cit.*, i, p. 223. *See also* index *s.v.* "Wissenschaft."

§ 2.

Recent psychology, recent logic, and recent speculation are at one in laying stress upon the solidarity between man's "theoretical" and "practical" activities. Without the implication of acceptance or rejection of the metaphysical contentions of "Pragmatism" we may usefully fall in with the prevailing fashion in Thought so far as to replace the current static conception of Science as a body of truths by a dynamic conception of it as a definite pursuit. Such a conception of it is adopted in this essay. Science is here conceived as a definite secular conative process which may be distinguished in and traced through the conscious life of civilisation. Only when a scientific "result" is thus considered in connection with the whole conscious process of which it is the "end" can we hope (as Mach taught us long ago) to submit it to profitable criticism. Since some such criticism is aimed at in this paper, it follows that either an attempt must be made to characterise that process or some current characterisation must be adopted as satisfactory. As I do not know one which I can accept as altogether suitable for my purpose, the former alternative must be embraced.

The statement that the conative process with which Science is identified reaches its end only in the enunciation of judgments of a certain class will probably be received without demur. Nor, if I say that these judgments refer to the Objective in experience will it be complained that I am ungenerously narrowing their field. The whole "furniture of earth and choir of heaven," "the starry heavens without and the moral law within" are but items in the inventory of the Objective. At the same time, although the Objective is here conceived as containing much more than "physical nature," it has its limits, and does not include everything that (in Mr. Bradley's phrase) can be set over against the self, and so become an "object" of attention; not everything that (because it

can be thought of) is maintained by Mr. Moore and Mr. Russell to have Being. A round square and Colonel Newcome are examples of objects of thought which are to be excluded from the Objective. We can, it would seem, characterise them by saying that these objects lack a certain priority to and independence of our thinking which is the necessary mark and guarantee of undoubted items of the Objective. It may be difficult to refute the argument that Colonel Newcome must have had being before 1854 or Thackeray could not have thought of him, but it will hardly be maintained that Thackeray discovered him "in just the same sense in which Columbus discovered the West Indies." At most it could be claimed (presumably) that "the elements so mixed in him" subsisted before Thackeray, by *selecting* them, brought them into a special relation in which they were not related prior to the act, and so "created" the Colonel.

The same distinction might have been indicated by saying that the Objective contains everything that must be "reckoned with," everything that must be considered as a *datum* for human action. From this point of view it is clear that although Colonel Newcome forms no part of the Objective, Thackeray's conception of him does, being a thing that has influenced human action on a comparatively large scale, and being precisely the conception it was and no other in virtue of its particular content, which must be distinguished from its object. Similarly, the Objectivity of my *thought* of a round square is demonstrated if it moves me to mirth or becomes so irresistibly attractive as to make me a "case" for the alienist.

But I am not disposed—at least without a struggle—to accept the position that this relevance to action is the essence of the Objective, and not merely a property of it coordinate with others. The present essay is in a sense a contribution to the discussion of this burning question, so that I will make here only one suggestion—a

suggestion that has probably occurred to many. If to be Objective means to have a relevance to purpose and action, how is it that we recognise material things and thoughts as having that relevance before the course of events has revealed it? Why do I ascribe Objectivity to the hundred thalers on the table, and deny it to the content of my thought of a hundred thalers before I have attempted to spend or even to touch them? The reply that it is because the former are like in all respects to thalers which *have* been things "to reckon with"—or upon, while the latter are not, is plausible in this special case, but does not seem sufficient to meet the general case of the recognition of Objective existents before experience of their relevance to action. Surely, we may retort, the perception of this similarity which is the signal of subsequent relevance to action is the direct and simple perception of the presence of Objectivity as such—a property which as a matter of fact is accompanied by the property of relevance to action.

In a somewhat similar way we may meet the contention that the Objective is that which is "the same for all." Upon this view the "finite centres" in which all experience occurs, find it at once possible, and necessary for the development of intercourse with other centres, to "pool" a large part of their experience, and this common matter becomes the objective world, exterior to all and the same for all. Some writers (*e.g.*, M. Poincaré) attribute a very great importance in this connection to language, which they seem to regard as actually the means by which a "same for all" comes to exist, and not merely the means of our becoming aware that it does exist. Here we may repeat our objection that the Objective is known directly as such prior to the discovery that it is the same for all. We may add in this case the further objection that in the inventory of the Objective we include not only unique experiences in the world of physical existents—such as astronomical observations incapable of repetition—but also the

whole world of psychical existents, whose very nature it is to appear in the "panorama" surrounding a single centre only. Not a single feature of such facts can be excluded from the operations of Science, yet how can they be regarded as "the same for all"? Only, I submit, by a kind of extrapolation from that part of the Objective—"physical reality"—which, as a matter of fact, *is* the same for all. We must say, that is, of such experiences, not that being the same for all they become Objective, but that being Objective (*simpliciter*) they are regarded as the same for all, and therefore, part of the proper subject matter of Science. Being experiences whose content announces itself as independent of the self of the moment over against which they are set, they may be thought of as occurring with an identical character in *any* centre. They become, that is, objects whose features, like those of "material objects" are capable of exact determination without reference to their presentation at all—although, of course, their position as a class of existents is fixed by their peculiar relation to the "finite centre" in which they occur.

§ 3.

The actual contents of the Objective must be reviewed very briefly. "Everyone except a philosopher," says Mr. Russell,* "can see the difference between a post and my idea of a post." I ignore this uncomplimentary reservation and assume that we are all prepared to admit not only that they are existents of different orders, but also that both have (like all existents) the character of Objectivity—the post, because it would be the particular thing which it is, even if I did not happen to see it; the idea because it would be an idea with just that particular content, even if I did not happen to perceive that I had "had" it. Difficulty only arises in the absence of the plain guarantee of "priority" which the perception of existence itself gives. In this case, to quote Mr. Russell again, "there exists

* Russell, *Principles of Mathematics*, i, p. 451.

everywhere the greatest confusion"—confusion that can only be removed by the frank recognition of another type of Objectivity which we may call *Objective subsistence.* If we think of the number 100, or of π, or of the tangent to an ellipse it must be recognised that the object of our thought has a priority to our thinking, that entitles it to be called Objective in the same sense as existents must be called Objective. Such objects of thought present themselves as features of experience which must be "reckoned with," and are not subject to our caprice. They may not be obvious to untrained inspection any more than the finer details of a microscopic section are, but when once envisaged by the competent mental eye they are observed to *have* their peculiar features as a matter of fact, quite apart from the observation. In Mr. Russell's forcible phrase such "subsistents" must be "discovered in just the same sense in which Columbus discovered the West Indies":—they are *Objective* subsistents.

Attention should, perhaps, be called to the fact that the foregoing use of the term Objective differs from the technical use of the same term recently introduced into Philosophy by Meinong and his school. (Meinong's works, *Ueber Annahmen,* 1902, and *Untersuchungen zur Gegenstandstheorie und Psychologie,* 1904, have been the subject of reviews and discussions by Mr. Russell, in *Mind,* N.S., Nos. 50, 51, 52 and 56). In the complete "Theory of Objects" an object (*Gegenstand,* "an object of discourse") is either an *Objekt* or an *Objektiv.* Thus (to borrow one of Mr. Russell's examples) if I pronounce the judgment "There was no disturbance," although I deny the existence of a certain *Objekt*—a disturbance at a particular time and place—I yet assert something positive, namely, the *fact* that there was no disturbance. This fact is the *Objektiv* of the judgment. So, if I assert that "A is the father of B," my judgment concerns the *Objekte,* A, B, and the relationship between them, while the *Objektiv* of the judgment is, once more, the *fact* of relationship asserted.

It is clear from this description that an *Objektiv* can never exist: it can at best have subsistence. On the other hand, *Objekte* may either exist, or have subsistence only, according to circumstances. The question whether all non-existent objects have subsistence is one of great difficulty as well as of great importance, but does not concern our present inquiry. It only arises with the consideration of the standing of such objects as the *Objektiv* of a false judgment, or an impossible *Objekt* such as the much discussed round square. Meinong denies the subsistence of such objects outside the psychical process whose content they form: Mr. Russell, on the other hand, asserts the subsistence of all possible objects, but defends himself against the necessity of admitting the subsistence of impossible objects by a theory of denoting which robs them of the character of objects. By this later development of his doctrine Mr. Russell appears to be able to bring himself into line with common-sense, freeing himself from the necessity of admitting the Being of imaginary entities like Colonel Newcome, or impossibilities like the round square.* Thus he leaves no *Objekte* except those which I have already described as presenting that priority to our thinking which the term Objective (as I have used it) is intended to connote. But Mr. Russell finds himself obliged to maintain the subsistence of all *Objektive*, whether of true judgments or of false†—even, apparently, in the cases where the "subject" of the proposition, in accordance with the theory of denoting, is not an *Objekt*. Thus, although the round square is not an *Objekt*, "das Nichtsein des runden Viereckes" is still an *Objektiv*,‡ and as such has subsistence. But subsistence must be granted also to "das Sein des runden Viereckes"; the only distinction which it is possible to draw between this *Objektiv* and the former being the (so to speak) external distinction of truth from

* *Mind*, No. 56, p. 491.

† *Mind*, No. 52, pp. 521 *et seq.*; No. 56, p. 532.

‡ Ameseder, in *Gegenstandstheorie*, p. 55.

falsity. It hardly needs remark that Science deals with *Objektive* as well as with *Objekte*—not only, for example, with zinc, sulphuric acid, and hydrogen, but also with *the fact that* zinc displaces hydrogen from sulphuric acid. But it will be recognised just as readily that it concerns itself only with *Objektive* which are believed to be *true*. Finally, then, the Objective, as here conceived, includes (1) all *Objekte* (according to Mr. Russell), or all that have existence or subsistence (according to Meinong); and (2) all true *Objektive.*

§ 4.

In the last section only the broadest outline was given of the classification of its objects which is implicitly made by common thought. But the Objective contains—and Science accepts at the outset of its task—much more than these very general distinctions. The most important of the details—which may be said to constitute the "plain man's" view of the world—are implied in the common structure of languages. This structure was elaborated doubtless in order to meet the exigencies of our transactions with the world of physical existence; primarily, therefore, it affords information about the view which mankind has universally taken of that order of reality. Briefly,* that view is that the physical world is a collection of *Things*, possessed of Qualities, occupying correlated positions in the great "continuous receptacles" of Space and Time, and capable of Activities, in the course of which the things may "change" in respect of their qualities, their mode of occupation of space or their spatial relations to other things, and may "cause" similar changes in other things. The critical examination of the notions indicated—notions which everywhere permeate ordinary expressions of experience—is part of the special business of Philosophy,† and cannot be

* Sigwart, *Logic*, I, ch. i. *Cf.* Sweet, *History of Language*, 1900, pp. 47 *et seq.*

† *See*, e.g., Hodgson, *Proc. Arist. Soc.*, N.S., IV, p. 2.

pursued here farther than is necessary to make it clear to what precise extent they are, in my view, to be accepted as having Objective reference.

§ 5.

It will be most profitable to pay attention first to the qualities of things, simply because subsequent criticism has tampered here so constantly and for so long a time with the unsophisticated deliverances of the "plain man's" consciousness that it is not easy to ascertain precisely what those deliverances are. This criticism has not merely erected a distinction between the "primary" and "secondary" qualities of things but has gone on to regard it as evident to all who take the trouble to avoid "a natural fallacy of ordinary thinking" that the former alone really belong to things, while the latter are, strictly, qualities of the sensations which things produce in us. In opposition to this I wish to maintain that the view actually held by the enlightened plain man is substantially the one presented and vindicated by Professor Stout.* Redness, sweetness, hotness, &c., belong to the body that "has" them in the same sense as extension, hardness and the other primary qualities belong to it. A hot piece of iron remains as hot even if I move away from it, just as it remains of the same length in spite of the diminution in the space which its image covers on my retina. It becomes hotter or colder only if it yields a different sensation under the same conditions of perception. In fact the plain man makes here—implicitly of course—the same distinction between changes of sensible appearance which have "representative value"† and those which have not, as he does in the case of changes which he interprets as changes either in the "position" or in the "form" of the object according as he is able or unable to

---

* Stout, "Primary and Secondary Qualities," *Proc. Arist. Soc.*, N.S., IV, pp. 141 *et seq.*

† Stout, *op. cit.*, p. 145.

reinstate the original sensible appearance by voluntary movement.*

It is true that a certain amount of difficulty is presented by commonly occurring cases in which the plain man's lack of interest in analysis leads him into apparent inconsistencies. Thus when it is said of a plate in a certain position that although it "looks" elliptical it is "really" circular, or of two coloured materials that though they "appear" not to match (by gaslight) they "really" do so; or, again, that the tablecloth which (in the dusk) "looks" black is "really" red; the well-known explanation that the word "really" refers here to assumed normal or standard conditions of perception is too often taken as carrying the further implication that *none* of the qualifications are Objective. In the case of the change of shape this destructive suggestion is successfully resisted by "everyone except a philosopher." Since the various shapes are so completely under our control and can be made to melt continuously into one another, we have no difficulty in admitting that they all are simply different representations of the same thing which actually *is* round, or elliptical, with an endless succession of eccentricities, according to the way in which it is looked at. Now the "plain man" will often make similar statements in the other cases: he will say naively that the materials which "are" of the same colour at noon "are" differently coloured by gaslight; that the tablecloth which "is" red in the daytime "is" black in the late twilight, and this view of the matter is, I believe, the one which the "plain man" would stand by if he were challenged; while he would be willing to admit the conventional character of the former statements. Further, it is only (I submit) in the light of special *scientific* attempts to "explain" these different manifestations of the same thing that it becomes regarded as

---

* Poincaré, *Science et Hypothèse*, p. 76. *La Valeur de la Science*, pp. 82 *et seq.*

"a natural fallacy of ordinary thinking" to credit such changes in colour to the things themselves.

But it is possible to confront the view that secondary qualities exist otherwise than as the contents of our perceptions with greater difficulties than these. It is a familiar fact that if a cyclist, A, rings his bicycle bell, B, whom he is approaching, and C, from whom he is separating, hear different notes—notes which are respectively higher and lower in pitch than the note which A hears constantly, and all three persons would hear if he were stationary. Can it be said that the bell is actually "emitting" these three notes simultaneously (not to mention the indefinite number of other notes which might be heard at the same moment by different passers by) but that only one of them is heard by each of the persons concerned? It must be admitted that the "plain man" who had found no offence in the thought that the same substance might be of different colours at different times under different circumstances would probably be alarmed at finding himself thus committed to the opinion that the same bell was giving out to different observers different notes at the same time.

The moment at which this discovery was made would be the moment at which a philosopher, cynically contemptuous of the "plain man's" confidence in his view of the world, would choose to point out to him that the thing which is hot or heavy to one man may be cold or light to another and would ask him whether he supposed that the hotness (or heaviness) could exist at the same time in the same place as the coldness (or lightness).

To meet these difficulties the "plain man" would have to become, himself, something not very different from a philosopher, and reply that these apparently inconsistent objects of sensation *do* all exist and, for that matter, in the same place, just as the different shapes of the plate all exist in the same place, although from each single point only one of them can be perceived. Here an indubitable philosopher might take up the "plain man's" case. Things which "have" the primary and secondary

qualities are not isolated reals, each one wearing the qualities it owns without regard to any other. Rather do they all form part of one spatial real, "the physical order," which includes our own bodies. Then it may be said in general that the qualities perceived to exist in any one part of the real are correlated with those perceivable in other parts—including that part (or "body") with which the perceiving "centre" has a special connection. Whether the same part of the physical real will be perceived as occupied by the same quality at the same time by two "centres of experience" or by the same centre at different times cannot be decided in any given case by *a priori* reasoning. Thus the colours seen in the same surface by daylight and gaslight are different, while the sounds heard from the same bell would not perceptibly change with the change of light. If a bell is sounded near different observers the sound heard will depend upon the velocities with regard to the air of the bell and the percipient's body, and it seems probable that it will depend *only* upon these velocities, so that if these are identical in the case of two observers, identical sounds will be heard. In the case of the hotness or coldness felt in a particular part of the real (for example, a basin of water), the quality perceived to exist depends upon other conditions which involve the percipient's body in what may be called an internal way. It is clear that this account may be extended to include such extreme cases as "colour-blindness," or complete absence of certain forms of sensibility. The all-important point is that we cannot deny to the qualities perceived the Objectivity, the "priority" to our perception that they claim to have in each case; scientific "explanations" of the phenomena consisting largely, I repeat, in bringing to light other Objective facts which exist *as well as* the primary facts from which the explanations start, and in no sense exist *instead* of these.*

* I need hardly point out how much I am indebted to Mr. G. E. Moore in this discussion. Mr. Moore dealt with the subject in a paper read before the Aristotelian Society in December, 1905—some months after, under his

Such an analysis as this undoubtedly demands a modification of the ordinary notion of a "thing" in accordance with which all its qualities occupy the same place. Thus, to revert to an example taken from Professor Stout's paper, if I move away from the fire I do not suppose that the hotness of the fire changes. At the same time my judgment that "it is not so hot here as there" does (I hold) refer to an Objective hotness which has "priority" to my perception, even if (as is conceivable) I am the only person who could perceive just that hotness at that place and time; it is a hotness which (in Mr. Moore's words) "exists just as my perception of it exists." We may interpret the "plain man's" implicit recognition that these differences of hotness "have no representative value," do not (that is) imply any changes in the actual "hotness of the fire," not by supposing that the different perceptions are really different perceptions of the same hotness, but that they are different hotnesses which exist at different places around "the fire"—that is around the place where the other (chiefly primary) qualities of the fire are to be found—and are correlated with the particular hotness which characterises "the fire itself." Upon such a view as this the "thing itself" is only the place where *some* of its qualities exist, while other qualities of the thing may be found throughout the region round about this place.*

§ 6.

It would be great presumption to hope to do more in a few pages than indicate quite broadly the manner in which the "plain man's" view of the Objectivity of the qualities of things, secondary as well as primary, might be defended

---

inspiration and that of Mr. Russell, I had reached the views given above. It is, I suppose, in accordance with precedent that a follower should prove "plus royaliste que le roi,"—a formula which seems to cover many of my divergencies from Mr. Moore's doctrine.

* This notion is, of course, not new. *See* Lotze, *Metaphysics*, ii, p. 34; Ostwald, *Vorlesungen über Natur-philosophie*, p. 193.

against philosophical attacks. The question of the *thing* which the "plain man" regards as "possessing" these qualities must be treated still more briefly and (unfortunately) in a form that may, on account of its brevity, seem dogmatic. The difficulties that have prevented philosophers from accepting the Thing as Objective are well known and need not be described.* It has not been found possible to conceive anything of the nature of a "core" which should at once be without qualities, yet be able to "have" or "support" the qualities which we know, and to persist through their changes. The attempt of materialism or of mechanistic science to account for the whole body of experience in terms of "matter" and "force" is so easily shown by metaphysical criticism to be a failure that it would not be mentioned here if it were not for its special relevance to the aim of my essay. From the point of view which I have adopted the capital fault of materialism lies, not in its belief that the primary qualities—or some of them—are Objective, but in its denial that the secondary qualities are so. It seeks to *replace* the presented facts of experience by other alleged facts of which the former are explained to be only appearances.

The hopelessness of this kind of solution of the problem of Thinghood (which has been known to Philosophy from the time of Berkeley) has led to a search for a better one in other directions. Typical of the kind of solution that has commended itself of late is Lotze's pronouncement that "it is not in virtue of a substance contained in them that things are; they are, when they are qualified to produce the appearance of there being a substance in them."† The "core of substance" has disappeared and we are left with an "empty shell" or (more soberly) a complex of sensible qualities.

---

* *See* Bradley, *Appearance and Reality*, Ch. VIII; Lotze, *Metaphysics*, i, Bk. I, esp. Ch. VII.

† *Metaphysics*, i, § 37.

When, after the destructive process has been carried to this point, the question how the qualities "hang together" so as to "present the appearance of there being a substance in them" is again taken up, the Objectivity of the thing—its priority to our perception—has, in the view of most philosophers, seemed untenable. Either the connexion between the qualities is regarded as sharing the inconsistency and, therefore, the unreality of other forms of "appearance"; or it is merely psychological. Upon the latter view "the concept of permanently existing things" is a great "common-sense achievement";* it is "a thought-symbol for a compound sensation of relative fixedness"—"the whole operation [being] a mere affair of economy."†

The view which is held without suspicion upon the plane of common sense can be saved upon the plane of metaphysical reflexion only by the recognition of the Objectivity that has already been claimed for at least some relations. If that claim be admitted it becomes possible to regard a whole which is a complex of elements in relation to one another as having Objectivity apart from the fact of the Objectivity of its parts. The "melody" which is heard when a succession of notes is played upon a musical instrument is such a whole; it consists of the notes in definite Objective relations and has to be "heard" as a presentation distinct from, though based upon, the presentations of the single notes.‡ In fact, it is possible

* James, "Humanism and Truth," *Mind*, N.S., No. 52, p. 461.

† Mach, *Science of Mechanics*, 2nd Eng. ed., p. 483.

‡ Ameseder (in *Gegenstandstheorie*, p. 498) calls this process "*Produktion.*" *See* Russell's review, *Mind*, N.S., No. 56, p. 537; *cf.* Browning's lines:—

And I know not if, save in this, such gift be allowed to man,
That out of three sounds he frame, not a fourth sound, but a star.
Consider it well: each tone of our scale in itself is nought;
It is everywhere in the world—loud, soft, and all is said:
Give it to me to use! I mix it with two in my thought:
And, there! Ye have heard and seen: consider and bow the head!

—*Abt Vogler*, VII.

for a musician to have an "imageless apprehension" even of a long piece in the process of composition, before the separate notes or chords have been presented at all.* There is little need to dwell upon the psychological aspects of this process, which have been treated in detail by Professor Stout.† It is necessary only to point out that by the admission of the possible Objectivity of a whole as a distinct entity subsisting *as well as* its parts and their relations, the Objectivity which common sense finds in the Thing, apart from individual qualities, can be conceded by the philosopher. The Thing "has" its qualities just as a melody has its notes.

This attempt to justify common sense, like the former one, is confronted by difficulties which cannot be overcome without taking the "plain man" through philosophic by-ways where he may feel that he has lost touch with the "distinctions that are plain and few" among which he moved at the beginning of his journey. Not only may a vast number of objects of sensational processes have to be thought of as belonging to the Thing although they lie outside the space within which its "primary qualities" manifest themselves‡ (the space which we call "the thing itself"); not only must the network of real relations which individualises the Thing present points of attachment for all the various *Objekte* which characterise different "states" of the Thing;§ there is the greater theoretical difficulty of deciding, since no part of the presented world appears to be independent of any other part, what precise relational network so isolates a group of presentations as to constitute them into a Thing.‖ It must suffice here to

* The description given by Mozart (quoted by James, *Pr. of Psych.*, i, p. 255) is well known: "I can see the whole of it at a single glance in my mind."

† Stout, *Analytic Psychology*, i, pp. 95–6.

‡ P. 15, *supra*.

§ Lotze, *Metaphysics*, i, p. 77.

‖ *Cf.* Bradley, *Appearance and Reality*, p. 71.

recognise these difficulties and to remark that though the second and third may make it impossible to apply the concept of Thing quite unambiguously throughout the field of physical existence, yet in the cases in which the "plain man" uses the notion in its primary sense, his confidence that he is applying it to Objective elements of experience is (upon the general view here adopted) abundantly justified.

A few words may be added upon the question of the perception of a Thing as distinguished from the apprehension of the sensational *data* upon the basis of which the Thing is "constructed" at the moment of perception. As described by psychologists* the perception of a Thing involves an "acquirement of meaning" due to the "complication" of the presented sensational *data* with other objects of the same kind, not at the moment presented. In terms of the explanation given above, the object of the whole psychical state is the complex whole of these past and present sensational *data* in their Objective relations. The recognition of the Thing is, then, quite analogous to the recognition of a melody from its opening bars, or of a poem from its opening words—with the difference that the whole in the latter cases has Objectivity only in a secondary sense as the actual content of an artist's invention.

But since the objects of the past sensations and a great part of the relational nexus are not actually presented at the moment, "illusions" or misinterpretations of the presented *data* are possible. This fact does not destroy the Objectivity of the Thing, any more than the incorrect identification of a melody from the opening notes implies want of Objectivity in the melody. The object of the whole percept is Objective, but it is not the object (it turns out) that ought to have been presented, but one which, as far as the part immediately presented is concerned, happens to be not easily distinguish-

* *E.g.*, Stout, *Manual of Psychology*, 2nd ed., p. 93.

able from the latter. This leaves outstanding the difficulty that two really different objects are not always distinguished; a difficulty which, though it has quite rightly been indicated* as a serious one for Realist philosophies of this type, is one which in a treatment that aims at little more than indicating clearly the philosophical basis adopted in this essay, I may perhaps ask leave to hope is not insuperable—while we pass on.

§ 7.

When we turn from the world of physical existences we find the "plain man" applying with less confidence the distinctions which are clear and easy throughout at least the greater part of the material realm. Interest in psychical existences arose long after language, under the influence of interest in the material environment, had developed its permanent essential forms. When attention became directed towards objects of a fundamentally different character the existing machinery of expression was utilised, with as little modification as possible, to deal with them. The result is that it is difficult to determine in the case of "things" which are not undoubted substantive material unities, how far the use of the term is simply conventional, implying no more than a possible "subject of discourse." Thus when the poet says of sleep that—

> "it is a blessed thing
> Beloved from pole to pole,"

it seems evident that the word is used conventionally beyond the original limits of its application, just as in algebra the notion of indices, which, in the original definition, must be integers, is modified to include fractional and negative numbers. How far the same thing would be regarded by the plain man as true in the case of psychical existences, thoughts, feelings, and the like, it would be hard to determine. Since, however,

* See *Mind*, N.S., No. 58, p. 231.

no question of the Objectivity of these elements of experience can arise, it is unnecessary to discuss the implications of the plain man's terminology in this region.

§ 8.

On the other hand we cannot pass from this part of our subject without noting the fact—of fundamental importance for Science—that the constituents of all three of the orders of the Objective which we have recognised may be considered as forming *series* in respect of many of their various characteristics.* Thus the pitches of a number of notes, their loudness, their purity, their "reediness," their squeakiness, the time during which they are prolonged, the distress they cause to various listeners are all examples of series. The peculiarity of a series is that (with the exception of two, the "first" and the "last") each term has to at least two others the relation which is understood when we say that it is *between* them. If a term x is between the terms m and n and is also between p and q it will happen that two of these four (*e.g.*, m and p) will both have the same asymmetrical relation to x, while x will have that same relation to n and q. In that case we may say that p and m *precede* x while n and q *follow* it. For example, if the asymmetrical relation in question be "more squeaky," all notes which are more squeaky than a given note will precede it; all notes than which the given note is more squeaky will follow. By considering each in turn we can determine the order of the series, the "first" term being that which precedes every other but does not follow any, the "last" term being that which does not precede any but follows all. It may happen that one or both of these special terms or *ends* cannot be found. In the former case the series must be *infinite* and in the latter it must be either

* The terminology and the main ideas of this section are borrowed from Mr. Russell's admirable and profound treatment ; *op. cit.*, Parts III, IV, V, VI.

infinite or *closed*—like the series of palings round a field. In addition, it may happen that two or more terms are identical in respect of the terms that they lie between—which implies that none of these particular terms is between another of them and any one of the other terms. In this case the terms in question occupy *the same place* in the series—it being carefully noted that no spatial ideas are intended by this or by similar expressions. In the case we have considered two notes which are "equally squeaky" occupy the same place in the series.

If we consider two terms a_1, a_2 of any of the above series there will in general be a number of terms between them: these terms, together with one of the two a_1, a_2, constitute what may be called the *stretch* from a_1 to a_2. To be distinguished from this notion is that of the *distance* of a_2 from a_1, an idea which must be kept free from spatial associations, although, of course, it takes its name from its analogy with distance in space. Thus in the case of two notes in a scale the distance would be in the "interval" between them while the stretch would consist of all the notes that could be intercalated. Distances, then, are asymmetrical quantitative relations between the terms of a series such that one and only one belongs to any given pair. In all the series suggested above there are distances as well as stretches. There are, similarly, distances between shades of colour, degrees of hotness, &c., but it is curious that, if after the consideration of such cases one returns to the series of positions in space or time, it is doubtful whether distance can still be detected in them!*

Again, it is possible to divide each of the suggested series into two parts so that one of the original terms becomes the "last" term of the first part, and another (the "next" term) becomes the "first" term of the second part; for between these two terms no further terms of the series occur. Series which can be treated in this way are *discrete*, and consist of a succession

---

* Russell, *op. cit.*, p. 255.

of *consecutive* terms. But a great many series which the Objective presents to us have a structure which will not submit to this simple operation. Thus in the case of the series composed of *all* sounds considered in respect of pitch it is impossible even to conceive the series as divided into two parts in such a way that one note is the highest of the first part, and another the lowest of the other part. It is true that we may determine that a given note—say "middle C"—shall be the highest (or last) note of the former part; but, whatever note is now taken as the lowest (or first) of the latter part, it is always possible to think of yet another note as intercalated between this note and the middle C. In other words no note can claim the title of the *next* note to C.

It is clear that this statement could never be directly verified, but is rather of the nature of a postulate to which common sense seems to have been led by considerations which amount to the following.* It is possible to have a note B which seems to be in unison both with A and with C when tried separately, while, notwithstanding this, A and C are perceptibly discordant. In order to avoid a contradiction analogous to the supposition that things which are equal to the same are not equal to one another, it is concluded that the note B is "really" intermediate in pitch between A and C but cannot be directly perceived to be so. There is no reason to suppose that in the same way other notes might not be intercalated without limit between A and B. Thus we reach the notion that Poincaré has called the *physical continuum*.

It seems possible that this name is not the best that could be chosen. If the above analysis is correct the special characteristic of these physical series as they are conceived is that it is always possible to find another term of the series between any two. Thus starting with two terms T_1 and T_2 the intermediate terms will be obtained by the constantly repeated

---

* Poincaré, *La Science et l'Hypothèse*, p. 34.

addition of a new one. This process cannot lead to the introduction of an *infinite* number of terms—in the accurately defined sense in which philosophical mathematicians use the word.* As a matter of fact such a series is called by Mr. Russell *compact*† and must be distinguished from the *continuous series* which implies an infinite number of terms between any two.

§ 9.

The most important single series for our purposes is the series of *numbers.* Among all the classes of things in the Objective we can detect classes of *similar* classes—that is classes between whose terms a "one-one correlation" can be established. This is true, for example, of the days of the week, the Hills of Rome, the Champions of Christendom, the Wise Men of Greece and the Wonders of the World. These and the other classes similar to them owe their similarity to the possession of a common property—or, as Mr. Russell puts it, to their relation to a common term—their *number.* This is the logical definition of the cardinal numbers and probably indicates also the psychological process by which they were discovered. It is obvious that they form a discrete series infinite in number, and generated by the constantly repeated addition of *one* to the first of them; while no place in this series is occupied by more than one term. It is clear that such a series may be correlated with any other discrete series. This correlation—the system of counting or marking with consecutive numbers—must at a very early stage of civilisation have replaced simpler methods of correlation used for the same purpose. A time came when the series of integers proved to be insufficient for the practical purposes to which numbers were applied, and had to be extended by the addition of the terms with which we are familiar as "fractions." There can

* Russell, *op. cit.*, pp. 356–7.

† By Cantor "überall dicht."

be no doubt that this extension was actually effected in connexion with the problem of *measurement,* which we have yet to consider; but it will be instructive to conceive a method which might have been followed which involves no reference to this problem.

Let us take a concrete example. We will suppose that we have undertaken the task of compiling a "subject index" to a library—a catalogue in which a reference is given to the books dealing with each subject. Separate sections of the catalogue will be devoted to the main divisions of the subjects represented, and it will be necessary to interpolate between these main headings, as the library developes, sub-headings referring to the portions of the catalogue which are devoted to the sub-divisions of the subjects. Thus Section 518 may contain the books on Analytical Geometry, Section 519 those on Aids to Calculation. Later it becomes necessary to distinguish sub-sections devoted to Plane Loci, Trilinears, Conic Sections, Loci in Space, Conicoids, &c. To mark the position of these smaller divisions we need to interpolate between the numbers 518 and 519 other symbols that will continue in this new field the correlative properties of the integers. We arrive at the idea of employing for this purpose the symbols 518·1, 518·2, 518·3, &c.,—symbols which are obviously not in possession, directly, of all the properties of the integers, but can just as obviously be used to continue their unambiguous correlative function. Thus the symbol 518·3 fixes the position of the sub-section dealing with Conic Sections quite unambiguously between the sub-sections dealing with Trilinears (518·2) and Loci in Space (518·4) respectively. When the books concerned with Conic Sections are themselves numerous enough for classification, we can arrange the correlation of the secondary sub-divisions without disturbing the existing distribution of symbols. It is necessary only to repeat the same device at a further stage, and to correlate the new sub-sections devoted (say) to the Ellipse, the Hyperbola, and the Parabola

with the symbols 518·31, 518·32, 518·33. It is clear that we have here a method capable of indefinite expansion; that we have hit, in fact, upon a device that enables us to convert the numbers, extended in this conventional way, into a *compact* series, in which it is possible to insert a new term between any two given terms without limit.*

Evidently such a series could be correlated with the terms of any other compact series—for example, the series of notes arranged in order of pitch. The cardinal numbers could be assigned in order to the notes of the diatonic scale based upon the lowest of the series, while the new members could be assigned in order as intermediate notes were identified—the numbers being attached, for example, to the strings or pipes which yielded the notes. If the numbers were assigned in the manner explained above, it would be impossible that a note identified as being between two already recognised notes should fail to receive a number indicating unambiguously its position.

§ 10.

But the invention of these subsidiary numbers actually took place, of course, in connection with quite a different problem—the problem of *measurement.* Among the contents of the Objective which we have considered as capable of arrangement in order or series, some are recognisable as *quantities,*—that is as possessing *magnitude.* Such quantities can be judged to be *equal* to other quantities when they possess the *same* magnitude, *greater* or *less* when they possess greater or less magnitudes. In the case of some series—*e.g.,* the series of tone-pitches or colours—the terms are not quantities; in the case of others—*e.g.,* the loudness and the

---

* This illustration is taken from a page of the *Subject Catalogue of the Science Library* in the Victoria and Albert Museum. The formulæ in Professor Peano's *Formulaire de Mathématiques* are arranged on the same plan.

disagreeableness of the notes—they are quantities. In all cases the *distances* between terms of the series (if they possess distance) have magnitude, while the whole composed of a stretch is also a quantity. The correlating of magnitudes—whether of terms or of distances—with the number series will present no new feature except in the remarkable cases in which it is possible to say that one magnitude is double of another.

It is not difficult to see that such a statement applies in the first instance only to numbers, and contains a reference to the process of *logical addition.* The books on the top two shelves in front of me as I write form similar classes: I could "tell off" a book from the top shelf against one from the second until the whole of the books were simultaneously exhausted. Both classes of books possess, then, the same number—which happens to be 18. By logical addition I can regard both sets of books as constituting one class, "the books on the top two shelves." The number of this class, which is the logical sum of the original similar classes, is 36. This number is called the "double" of 18. This simple case illustrates the property of numbers which, besides their ready accessibility to all, and the ease with which they lend themselves to correlation, is the most important which they possess for the purposes alike of practical life and of science. The essence of this property is that when correlation has been effected between any terms of series and appropriate members of the number series, it is possible to predict the effect of carrying out a given operation on these numbered objects by mere consideration of the properties of the terms of the number series—the result of the operation in question being always indicated in an interpretable way by its correlation with the single number which is the result of the operations that have been performed on the numerical *data.* Thus we can predict without actual trial the number of pieces of paper, each 1 foot square, which it is possible to place on a rectangular area measuring 4 feet long and 3 feet wide.

The simplest case of measurement is that of stretches. Even when the terms are not quantities, if the number of terms in one stretch is double that in another it is natural to consider the first *stretch* as double of the second. This is our meaning, for example, when we say that one man's flock of sheep is double, or twice as great as, another's.

In the case of terms which are quantities, measurement would seem possible only when each term can properly be regarded as the sum of parts. This is not true, for example, of pleasures. If it gives me equal pleasure to entertain A and to entertain B, to entertain both together gives either two pleasures—the former two—or else a single new pleasure—the pleasure of entertaining A and B together—which cannot be considered as the sum of the former pair. Nevertheless when, as in this case, the physical source of a quantity is duplicated there is a great and natural tendency to consider the quantity to be doubled also. Thus most people would consider the loudness of two equal notes to be twice the loudness of one. It is hardly necessary to quote Leibniz's doctrine of *petites perceptions* to show how strong the tendency is, and we shall find important examples of its influence in science.*

In the case of distances we meet with a similar difficulty; for the distance from a term A to a term C cannot strictly be regarded as the sum of the distances from A to an intermediate term B and from B to C. Here, however, it seems even more natural than in the last case to consider the distance AC *twice* the distance AB or BC if these are equal. Thus the interval from the note C on the piano to G is equal to the interval from G to D in the next octave. This being the case, we may consistently regard the interval from C to D as twice the interval from C to G.

There remain the cases of Space and Time, in which without doubt we have wholes composed of parts. Here, though

* *Infra*, p. 97.

an element of convention is still present (for the *magnitude* of the whole can hardly be regarded as the sum of the magnitudes of the parts), we judge one magnitude to be double another if the quantity which has the former magnitude can be divided into two quantities whose magnitude is the same as that of the latter.

In the cases considered, then, we can, by the aid of certain more or less easy conventions, solve the problem of measurement. That is, we can so correlate the members of the number series with the members of the series in question that whenever the numbers are twice, thrice, four times one another, and so on, the corresponding quantities or stretches or distances will be in the senses indicated, twice, thrice, four times one another, and so on. In every such case we may make use of the correlated numbers for the purposes of practice or theory in the manner already indicated.* It is clear, moreover, that by conventions which contain the familiar rules for the manipulation of decimal "fractions" we may apply to this purpose the whole of the extended "compact" series of numbers that was reached in the last section.

§ 11.

It is a commonplace that the series of Time and Space play an all-important part in Science; it will be desirable, therefore, to consider briefly certain special characteristics and special difficulties which they present. We can dismiss in a few words the property possessed by the spatial series of having more than one "dimension." From the critical standpoint it is chiefly important to note that this is not a unique property. The notes played by a performer on the pianoforte may be

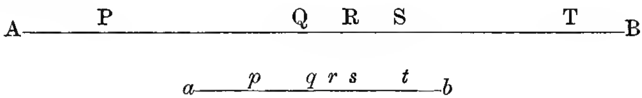
* It should be noted that measurement of "magnitudes of divisibility" such as Space and Time and (conventionally) Weight require a *unit*—a part which is repeatedly applied in order to measure the whole. Such measurement involves the assumption that during the successive applications of the unit its magnitude remains unchanged.

considered to present us with an example of a two-dimensional series with which the three-dimensional spatial series may be profitably compared. Each of such notes would be characterized at once by a definite loudness and a definite pitch, and could be described in an unambiguous way, so as to distinguish it from other notes of the same pitch but of different loudness, or of the same loudness but of different pitch, only by some device that would express at the same time its position in both the simple one-dimensional series of pitch and intensity. This could be done either by assigning to the note two distinct numbers, or "co-ordinates," one to fix its position in each scale, or else by extending the notion of number still further to meet such cases. Thus such a symbol as 256P+8L might conventionally mean that the note had the pitch corresponding to a "frequency" of 256 vibrations, while on a certain definite scale its loudness was 8. It is clear that both of these devices may be used to mark the position of points in space with regard to three mutually rectangular planes. The former is the device of rectangular Cartesian co-ordinates, the latter gives the series of "complex numbers."

§ 12.

At once of more importance and of more difficulty are the questions connected with the *continuity* of Space and Time. It is clear that both these series are *compact*, that is that we find ourselves bound to think that between any two points of spac another point exists, and between any two moments of tim another moment. No special problem arises here, for we have already seen how the original number series may be extended so as to provide for the correlation of any such point or moment with an unique number. Difficulty first appears when we discover that the number of points between any two points, A and B, must be thought of as *infinite* in a sense in which we cannot apply the term to the number of points which we could conceive as correlated with numbers of the extended number

series. This sense, which it has been reserved for the modern philosophical mathematicians to discover, must be briefly explained.



The straight line AB may be supposed to be the road which an Oriental despot has driven across his empire from one great city to another, while the shorter line *ab* may be taken as a map or plan of this road showing at the points *p*, *q*, *r*, *s*, *t*, the relative positions of the towns and villages, P, Q, R, S, T, to be met with on the way. It will be admitted that though the imperfection of human powers would make it practically impossible to represent every feature of the road AB on the plan *ab*, yet the difficulty is only a practical one, and that there is no detail of the road that could not conceivably be represented on the plan. The obvious consequence of this admission is that every point on the line AB can be correlated with a point on the line *ab*. According to the idiom which we have employed before, the two series of points are similar. But the line *ab* could be placed so that its points would become actually a part of the points of the line AB. Thus we should be faced by the disturbing conclusion that a part of these points may be similar to the whole. The number of points, then, on the line AB is such that a part of these points is capable of one-one correlation with the whole. But however far we carried the assignment of terms of our compact number series to the points on the line AB, it is perfectly certain that some of these numbers could not be correlated one by one with the whole. We are driven, then, either to the conclusion reached by such philosophers as Mr. Bradley that space is an irrational idea, "riddled with contradictions," or else to the provisional adoption of the concept of a new type of numbers whose

distinctive property is to be that they are the numbers to be assigned to the terms of a series when the part is similar to the whole. For such numbers we may reserve the term *infinite*. Fortunately we are not compelled at this stage in the progress of science to accept the former of the alternatives just offered. The consequences of the admission of a type of "infinite" numbers having the property explained have been carefully traced out by competent thinkers, and the contradictions which would, on Mr. Bradley's principles, condemn the concept have not appeared; while, on the other hand, large provinces of fact have fallen into order under the application of the, at first sight, startling concept which we have reached.

But before the ideas which we have just been considering had shaken off their scarcely reputable association with Eleatic paradox, and had entered into the sober service of the modern mathematician, it had been found that the compact number series was inadequate to the demands that geometrical theory put upon it. Thus, if the two sides of a right-angled triangle are equal in length, then the number which geometrical theory compels us to correlate with the length of the hypothenuse is not one of the compact series. If the number one is correlated with the magnitude of the side, the number which should be correlated with the magnitude of the hypothenuse is one whose "square" is two. It is easy to satisfy oneself that no such number is included in the compact series. Two alternatives arise. Either we must conclude that numbers are inadequate to the concept of space which our experience forces upon us, or else we must arrive at some means of supplementing the number series still further by means of clearly definable "Objective subsistences" which will be available for correlation with the newly discovered magnitudes.

As a matter of fact, it seems doubtful whether we can define with logical rigidity numbers with the required characteristics that can be regarded as interpolated between the terms of the

existing compact series of "rational" numbers.* It appears to be necessary to have recourse to a totally new series of concepts which, though derived from the rational numbers, are not identical with them. These are the "real" numbers of the mathematician. Their nature may be indicated as follows :—

1 2 3 4 5 6 7 8 9 10 11 12 13

In the line above are printed the symbols of the integers from 1 to 13, irregularly spaced so as to emphasise the fact that we are required to attend only to their order, not to any notion of distance of a spatial kind. Hitherto such a number as *nine* has been regarded as the common property of an indefinite number of "similar classes," each of which would, in ordinary parlance, be said to contain nine terms. But, given the definite ordinal series of integers, thus defined, it becomes possible to regard nine as defining a *class* of integers, namely, the class containing the integers 1 to 8 which precede it. In this way each of the integers can be correlated with a class of integers, so that "counting," say, of the persons round a dinner table, may be conceived as correlating them, one by one, with the *classes* of integers with which the symbols 1, 2, 3, &c., are also correlated.

It is clear that nothing in this will become invalid if we include in the classes of numbers, which are henceforward to be our instruments of correlation, the whole of the rational numbers which have received rigorous definition. Thus we can suppose our dining guests to be correlated, one by one, with the successive classes of rationals less than one, less than two, less than three, &c.; and there will be no danger lest the correlation should be incomplete or ambiguous. It is true that our ingenuity might run the risk of comparison with that shown by the White Knight if we deliberately employed so far-fetched a method in so simple a case as the counting of guests round

* Russell, *op. cit.*, p. 270.

a dinner table; but we should reap the reward of it when we came to deal with such questions as the correlation of the members of some convenient series with such spatial magnitudes as the sides of the triangle recently under consideration. For it will readily be seen that if we correlate the magnitude of the sides with the class of rationals less than one, we may quite unambiguously correlate the magnitude of the hypothenuse with the class of rationals whose squares are less than two—since no doubt can ever arise as to whether a given rational number falls in this class, nor confusion of limits with another class be feared. In this way the "irrational" numbers which are necessary to enable us to correlate the terms of the number series with all the points of space that anyone has ever found himself led to postulate can be supplied.

It need hardly be added that by an appropriate symbolism—which is strictly a non-logical matter—these new members can be made to perform the services in which we have seen the great practical importance of numbers to lie.

The difficulties that beset the measurement of Space were imported into Time when attempts were made to consider the question of Motion philosophically. Hence arose the famous paradoxes of Zeno. But there is no reason why Time should not be conceived as having the same kind of "continuity" as Space, so that for the correlation of the moments of Time, as for the correlation of points of Space, we must have recourse to the "real" as distinguished from the "rational" numbers. Motion then loses its "contradictory" character; for we can regard a "material particle" as simply a means by which correlation is set up between (1) a single point of Space and a number of successive moments of Time, if the particle is "at rest"; and (2) a series of points and moments, one by one, if the particle is "in motion."*

* For details *see* Russell, Ch. LIV.

§ 13.

On the fascinating subject of the Objectivity of Space and Time, our remarks must be very brief, and, as before, limited in the main to indicating the grounds upon which it would seem that the existence of absolute Space and Time must be admitted, without any serious attempt to elaborate the arguments in favour of this view, or to annihilate those which make against it.

The latter may be considered under three headings. The first of these would contain the philosophical arguments which aim at discrediting continuous Space and Time by showing that they are self-contradictory concepts. Mr. Bradley's attack is too well known to need description here, and is typical of this type of objection. The essence of it is that Space and Time demand terms and relations between these terms, while any attempt actually to exhibit these terms involves the investigator in a hopeless "infinite regress." But in the light of the mathematical theories that we have been sketching, it becomes clear that the attempt in question fails because it is undertaken with inadequate means, not because it is essentially hopeless. In the successive dissection of portions of Space in the hope of finding at last the indissoluble point, one is doomed to failure by the fact that in this way one can never arrive at an *infinite* number of operations—taking the term in the sense that has been explained. Nevertheless, we *are* able in the manner we have indicated to conceive an infinite number of atomic points, in which we can recognise the final terms which we seek, and to do so without finding ourselves involved in the contradictions which beset us when we embarked upon the quest armed only with an improper concept of an "infinite number." Thus the classical *à priori* arguments against points are successfully resisted.

The other two classes of argument aim at showing that the assumption of the existence of absolute Space is unprovable and

unnecessary rather than that it is demonstrably false. First will come the psychologists who point to the manner in which our concept of Space as "a single continuous receptacle" arises from our perceptual experiences as indicative of its purely psychological value as a great "economical invention." An interesting argument of this type is that developed by Professor Poincaré, who holds that our three-dimensional space is really nothing more than a conventional reduction—now hereditary in the race—of the indefinitely numerous "dimensions" of an extensive character which our muscular sensations must originally give us.*

Finally come the arguments based on the "descriptive" view of Science. Thus to M. Poincaré, faced with the old question whether the earth revolves or whether the firmament undergoes a diurnal revolution about it, the two propositions "'la terre tourne,' et 'il est plus commode de supposer que la terre tourne,' ont un seul et même sens; il n'y a rien de plus dans l'une que dans l'autre."†

When faced with the fact that results of scientific research —such as Newton's law of mutual accelerations, the law of "centrifugal force," and certain results of modern electrical theory—seem to be meaningless in the absence of absolute space, these epistemologists reply that such theoretical results are simply means by which we can analyse and describe the actual behaviour of the universe as it is given to us: and that we have no warrant for applying these descriptive instruments to hypothetical cases in which alone they could be thought of as proving the existence of an absolute space. Thus (they argue, in effect) although you may apply with success to the behaviour of the water in a rotating bucket the descriptive rules which you reached by analysis of the behaviour of the moon, yet your success is dependent upon the fact that you worked in the

---

* *Science et Hypothèse*, p. 73.

† *Op. cit.*, p. 141.

same universe on each occasion. It does not warrant a prediction of the behaviour of the water in the bucket if the whole of the rest of the universe were removed—even supposing that it is legitimate to assume that such a thing could happen.*

It is plain that such arguments cannot *disprove* the existence of absolute space. They cannot, that is to say, prove that it is *meaningless* to suppose the whole universe to be moved, each point of it, 1000 miles from its present place, along lines parallel to the line joining, at a given moment, given points on the earth and the sun. It seems, then, that we may fairly apply an argument which Mr. Bradley has on more than one occasion claimed the right to use, and has expressed in the epigrammatic form, "What is *possible* and what a general principle compels us to say *must be*, that certainly *is*."† The arguments of Newton, and the similar modern ones to which I have alluded, are reasons why absolute space "must be," which, as Mr. Russell remarks, still await refutation. Meanwhile fresh difficulties may be accumulated for the philosophers who take up, under the distinguished leadership of Mach, the extreme empiricist position. We have already seen that these writers maintain that Newton's concept of action between mass-points is merely a useful mode of analysing for descriptive purposes certain events that are in the habit of taking place in our corner of the actual presented universe; and that we have no right to suppose that it would prescribe the behaviour of another material universe, nor even of the present one if its configuration were to be modified to a serious extent. This doctrine of the economical function of scientific concepts is not in itself necessarily incompatible with absolute space, but the temptation to extend it so as to give an account of the origin of the two "single continuous receptacles" which contain the whole manifold of human experience is one which

* *Cf.* Stallo, *Concepts of Modern Physics*, p. 200.
† *Op. cit.*, p. 196.

this school makes no attempt to resist. The argument is presented in different forms. Thus Professor Karl Pearson teaches that perceptual space is simply "one of the ways in which we perceive things apart. There is nothing in the sense-impressions themselves which involves the notion of space." Space has, then, the kind of existence that belongs to the *order* of an arbitrary arrangement of letters, such as the alphabet—a kind of existence that must be carefully distinguished from the *real* existence, in their possession of which the letters may be compared with the groups of sense-impressions which we term objects.*

It is manifest that Professor Pearson's "mode of perception" is little, if any, more than Kant's "form of intuition" rebaptised. As is well known, Professor James has pointed out the entirely gratuitous assumption involved in this account of the manufacture of perceptual space in a mythological "Kantian machine-shop" of the mind,† and has devoted one of his finest chapters to the defence of a genuine psychological theory of the perception of space. According to this theory there is an element of crude "extensity" or voluminousness present in many, if not in all, sensations—an undifferentiated root from which the spatial order of perception of adult life is developed. The actual problem, then, is to understand how we arrange these at first chaotically given spaces into the one regular and orderly "world of space" which we know.‡ There is no reason why these spatial data, merely because they are given and many, should piece themselves together into one continuous whole; and, indeed, nothing is easier than to convince oneself, by observation of one's total state of consciousness at any moment, that the spatial elements of the various contributaries to that total state—"the sound of the brook, the odour of the cedars, the comfort with which my breakfast has

* *Grammar of Science*, 1st ed., 1892, p. 185.
† *Pr. of Psych.*, II, p. 273.
‡ *Op. cit.*, p. 146.

filled me, and my interest in this paragraph—all lie distinct in my consciousness, but in no sense outside or alongside of each other."* They enter into this special relation with one another only when they come to be "discriminated as parts from out of a larger enveloping space."† This discrimination is rendered possible in many cases by a difference in the "immanent sensibility" of the different parts of the sense organs, so that each "carries its special sign,"† while it is greatly aided by movements, either of the object or of the sense-organ. Given that we have in this way analysed a number of original vague vastnesses into a mass of correlated spatial details, the problem now arises of summing these sense-spaces into what we commonly think of as "real space." This process occurs through the mediation of constant external *things*, with which different sense-organs, or different parts of the same sense-organ, come into relation from time to time.‡ Through the long-continued commerce of the sense-organs with these constant external things we come either to regard different spatial feelings as equal to one another, or to select certain ones as feelings of the real size and shape of things, and to degrade others to the status of being merely signs of these.§ When, as in the case of the mouth, we have what we may think of as an external thing with which a special set of sense-organs—those of the tongue—has dealings, the thing in question comes to form "almost a little world in itself,"‖ whose spatial values have never been reduced to agreement with those yielded by the other sense-organs. This fact is readily exemplified by the simple experiment of exploring the teeth with the tip of the tongue, and then repeating the investigation with a finger-tip.

---

* P. 146.
† P. 147.
‡ P. 167.
§ P. 181 *et seq.* P. 269.
‖ P. 181.

However vigorously we maintain the principle that logical values are independent of psychological genesis, it is difficult to hold that this theory of spatial perception has no relevance to the question of the status of conceptual space. Even if we regard the process described by Professor James (or the similar one described by Professor Ward) as merely the elaboration of a crude "nativistic" spatial perception,* we seem to be obliged to admit that adult perception gives us not space but *spaces*, which in some cases obstinately refuse to merge their individuality in our one "continuous receptacle." What, then, is the precise relation of conceptual space to these diverse perceptual spatial entities? Granting with Professor James that Kant's account of the genesis is "pure mythology," are we compelled by the better founded theory which we owe to modern psychologists not only to recognise, contrary to Kant's opinion, that the space of geometry *is* an empirical concept drawn from experience; but to abandon also his argument that space is not a general concept formed by abstraction from particular presentations because it is "essentially one" and claims to refer to that which contains all perceptual spaces as part of itself?† May not the notion of a "continuous receptacle" after all be only an abstraction, a result of conceptual analysis performed upon the relatively isolated and self-contained spatial wholes which psychological inspection reveals; while the unique character which we ascribe to conceptual or geometrical space represents a pragmatical simplification of the actual *data* which is justified only by its fruits?

These questions involve the consideration of the wider question of the nature of concepts and their relation to percepts. Here one of the essential points appears to be that the concept and the percept are alternative ways in which the Subject deals

---

* Stout, *Manual*, 2nd ed., p. 377.

† *Critique of Pure Reason*, pp. 23, 25.

with the same Objective elements. Thus my concept of the blazing fire in the next room enables me to maintain practical and theoretical relations with it at times when none of its qualities are contents of a percept of mine. There is, then, in every concept a reference to Objective elements, though it is only in perception that these elements present themselves with a guarantee of their Objective character. To employ Mr. Bradley's well-known idiom, in perception reality is qualified by an ideal content, while in conception the ideal content is a "floating adjective" of reality—floating, however, as a captive balloon floats, with a subtle line still holding it fast to the ground from which it arose. In acts of imagination—when I picture a giant whose favourite food is bread and butter sprinkled with light brown horses,* or when with circumstantial detail I present the life of Colonel Newcome; in play—when the make-believe messenger arrives fiery red with haste astride of the nursery chair;† in authentic narrative of the past; in expressions of command; in plans or resolutions for future behaviour; in all these we have a synthesis of conceptual elements which, though it follows perceptual models, appears to owe its particular form to the initiative of the thinker. And by this ideal content reality is in each case qualified just so far as the primary facts of the moment will permit the qualification.

If this doctrine is true, then there is no such definite gulf between concepts and percepts as is frequently supposed. There is always a reference of the ideal content to the Objective world. In perception the Objectivity of the concept is self-announced and self-guaranteed: in *mere* conception there is no such guarantee of the Objectivity of the ideal content, but simply absence of rejection by the presented Objective of the proffered addition; while in negation there

* An achievement (I believe) of the late Professor Clifford.

† Stevenson, *Child's Play* (in *Virginibus Puerisque*).

is a rejection of conceptual elements which have their proper home in other Objective contexts.

In particular this doctrine seems to throw doubt upon the completeness of the distinction between perceptual and conceptual space. Conceptual space alone, we are told,* contains points, curves, and surfaces, and is infinitely divisible. These notions are reached as the result of carrying to an ideal limit either suggestions of perception or operations possible only to a certain extent with perceptual material. Upon our view the relation between spatial percepts and concepts should be stated very differently. Our concepts of the relations which we afterwards know as continuous curves and surfaces neither are suggested as ideal limits of perceptual *data* which are self complete, nor are introduced *ab extra* to render the *data* intelligible (as we shall afterwards maintain of the conceptual hypotheses of science); they are actual constitutive elements of the percept, and can be thought of apart from the perceptual whole only in the same way as I may think of the greenness of the apple that I declined yesterday, or the loudness of the cry which disturbed me an hour ago. Again, such a concept as the straight line is not, upon the view here held, gained as the limit of the process represented by the whittling of a stick. On the contrary, it is the concept of the straight line that gives to the perceptual *data* the unity which makes them stages of one process. Finally, we may ask whether the process (which Professor James describes) of analysis of a vague given spatial whole into its relational details does not imply in the same way the presence of conceptual elements isolated afterwards as the straight line and point. The fact that the point cannot be *perceptually* isolated is no reason why it should not have a kind of priority to the perception of areas which makes it in a certain real sense the means by which areas are perceived. The concept is employed, so to speak, in its intension, not in its extension.

---

* Pearson, *op. cit.*, p. 203.

The rejection of some such view as this seems to entail the adoption of one which makes an almost complete divorce between the conceptual and perceptual worlds. It must, in effect, be held that man was in the beginning thrown naked and helpless into an unintelligible and intractable world which as yet he had no means of reducing to his will:

> *οἳ πρῶτα μὲν βλέποντες ἔβλεπον μάτην,*
> *κλύοντες οὐκ ἤκουον, ἀλλ' ὀνειράτων*
> *ἀλίγκιοι μορφαῖσι τὸν μακρὸν βίον*
> *ἔφυρον εἰκῇ πάντα.*

So it was until at length Prometheus (disguised, perhaps, as Natural Selection) brought relief in the shape of the gift of a set of concepts linked each to all in the bonds of apodeictic certainty, and having the curious property that by the mere consideration of the relations of these concepts man could master, at least as far as his practical needs required, much of the bewildering flux of the perceptual world.

In a well known article,* Mr. Russell has, in effect, shewn that the denial of absolute space is equivalent to the acceptance of views of which the above is at most an exaggerated statement. In the relational theory conceptual space consists solely of the concepts of the relational nexus in which the "things" of perception are imbedded. The "points" which are thought of as the terms of these relations are only symbols of the things themselves. For this reason (possibly) Mr. Russell calls them "material points." He proceeds to point out that "in order to give an account, which will consist with the facts, of the intersection of figures, the order of lines and planes, and the nature of areas and volume," we must be prepared to assert either that "material points" are as a matter of empirical fact distributed just as spatial

* "Is Position in Space and Time Absolute or Relative?" *Mind*, N.S., No. 39.

points are distributed in the absolute theory, or else to supplement the existing distribution of these by a supply of "possible material points" which are indistinguishable from the real spatial points which it is sought to exclude. If we are to avoid both horns of the dilemma we must fall back upon that severance of the worlds of thought and perception which has already engaged our attention. The truths of geometry will then hold only of conceptual space, and their approximate validity in the perceptual world is really a matter of lucky coincidence, however it may have come about.*

As in other cases to be considered in the sequel the attitude which a thinker will take up on such a question as this will depend very largely upon his intellectual temperament. If he shares with Professor James and his fellow Humanists a constitutional distrust of "all noble, clean-cut, fixed, eternal, rational, temple-like systems" of thought he will probably hold by the relational theory of space, and glory in the name of empiricist which is hurled at him as a reproach. If, on the other hand, his faith in the organic unity of all truth is sufficiently robust to push logical arguments dauntlessly to their ultimate conclusions even when they seem to the Humanist "oddly personal and artificial," or even "bureaucratic and professional in an absurd degree,"† he will be an absolutist. Such a thinker will prefer to leave the difficulties presented by the psychological analysis of space perception to be reconciled with his main convictions by some theory of error.

In the article to which reference has just been made, Mr. Russell disposes of the question of the status of moments

* Our argument is not affected by the consideration that, after all, our space may be really of a non-Euclidean pattern, so that it is only approximately represented by the usual homaloidal model. This would mean that we fail to discriminate correctly the actual spatial relations which are presented to us. It could not negative the arguments in favour of the Objectivity of points.

† James, *Humanism and Truth*, p. 467.

of Time by an argument the force of which depends still more directly upon the conditions that we have discussed in the last paragraph. There can be no doubt (so runs the argument) that events occur simultaneously ; that is, that they occupy occasionally the same place in the temporal series. But it is impossible to conceive that this may be so unless their place in the series is fixed by their correlation with a single term of some independent series. This independent series with which the events of the world are correlated (just as the matter of the world is correlated with points of space) is the series of absolute moments of time.

CHAPTER II.

§ 14.

In attempting to exhibit the main outline of the Objective as it appears to the "plain man" before the advent of scientific interpretations, one runs the risk of an accusation of merely adding to the inhabitants of the shadowy land, where the "economic man" and the "natural man" who enters into "social contracts" already dwell. At the least one may be met by the objection that many or all of the plain man's "views" are, after all, interpretations—interpretations which themselves at one time represented the high water mark of "scientific" investigation. The objection undoubtedly has force and we must return to it later,* but the accusation may be evaded by the admission that the plain man as such *is* a fiction in so far as he is an abstraction from within the wider self of each of us. Much as the total outlook of mankind upon the world varies from China to Peru, there seems to be a solid core of agreement everywhere which alone truly answers to the description which we have given of the Objective. The scientific traveller on a high plateau of the Andes and his native guides view in different ways the impossibility of getting their potatoes to cook.† To the latter the impossibility is due to the simple fact that "the cursed pot," doubtless owing to the devil in it, "did not wish to cook potatoes"; to the former it is an interesting example of the dependence of the boiling point upon the pressure. But although the whole "situation" may be very different in the two cases, there is yet a common basis of inevitable *fact* upon

* *Vide infra*, p. 138.
† Darwin, *The Voyage of the "Beagle."*

which the scientist and the native (if he is intelligent enough) can see that their "animistic" or "scientific" *interpretations* are simply embroideries. If (remembering at this point that there exists a science of psychology) we say that the "things" before our travellers—the fire, the pot, the lukewarm yet boiling water, the unsoftened potatoes—are all of them "constructs," we must admit at the same time that they are *inevitable* or *primary syntheses* which mankind everywhere would make from the same sensational data, while the whole situation as it exists for the two men is a *secondary synthesis* which, when one's attention is called to the matter, is seen *not* to be inevitable. Wherever the "objects" of attention dealt with in the former chapter must be held to have a synthetic character, only these primary syntheses were intended. Adopting this distinction we may say that the scientific process is one out of several possible alternative processes by means of which primary facts may be submitted to further construction; and it will be recognised as true that the object of this secondary synthesis is to make the primary facts *intelligible.* But this characteristic, though of fundamental importance, does not suffice to distinguish the scientific from all the alternative processes contemplated. To assert that a thing is intelligible or that it has meaning is to imply that it forms an element in a *system* of terms in relation. Thus a word—for example the word "button"—standing alone has meaning chiefly in so far as it is recognised as belonging to the English vocabulary within which it may be either a verb or a noun. When I say, "Pray you, undo this button," the fact that the word is now brought into relation with other words in a definite system gives it a fuller but still incomplete meaning: I may mean a coat button or a door button. The doubt can be resolved only by the context, that is, by the position of the sentence in a still wider synthesis. In this way the request, "Pray you, undo this button," may have all manner of meanings from the trivial one which a common

domestic context would give it, to the profound and pathetic significance it has on the dying lips of King Lear.*

The point in question could be illustrated indefinitely, but it seems necessary to note only that in every case the "system" in which an element finds its meaning must ultimately be an apperceptive system. This term, although it appears to have lost its former vogue in psychology, is yet, perhaps, the best available to suggest the integral, the vital connection of such systems with the past experience and present interests of an individual consciousness—the connection which is part of what I have already sought to suggest by speaking of Science as a conative process. No treatment, in fact, which isolates the efforts that have generated Science from their psychological *milieu* can hope to do justice to its subject, the true nature of which can only be brought out by placing the scientific process in its proper position in a Natural History of processes which all aim at rendering the Objective intelligible. Only by following such a method is it possible to reach a clear understanding of the relations to one another of the various elements which a cross section of contemporary scientific thought would exhibit.

§ 15.

Among the interpretations of the Objective which demand comparison with the scientific, the most important from the point of view of distribution is "animism," the system of beliefs upon which are based those practices of "magic" which not only are found to-day under curiously similar forms among all savage races, but also have preceded the existing modes of thought among all civilised peoples. Indeed, the researches of authors like Professor Frazer† have revealed these ancient ideas still persisting widely beneath the modern

* Act v, scene iii.

† J. G. Frazer, *The Golden Bough*, 2nd ed., i, p. 74.

intellectual surface, and have even seemed to justify a fear lest the depths should some day be upturned and the results of centuries of man's toils and genius be overwhelmed. Moreover, they have shown that magic, so far from being an unorganised collection of bizarre superstitions, has every claim to the title of a logical intellectual system based upon fundamental principles, to repudiate which would be at the same time to repudiate science.* At the base of the whole structure we find the "scientific" Principle of Uniformity, which differs from the fundamental principle of Logic, that "of the same the same is always true,"† only by the addition of what we may, perhaps, call an "existence postulate" that "the same" for the purpose of predication actually occurs. As Dr. Frazer points out,‡ the principle takes the special form of arguments based either upon Similarity or upon Contiguity. Thus to secure the destruction of a distant foe, you procure a waxen effigy of him, and submit it to slow-roasting or to other ill-treatment, in the confident expectation that the unfortunate original will suffer analogous torments. Your hope springs, of course, from the belief that the two cases have a "core of identity" sufficient to make the "substitution of similars" effective. Again, if you have succeeded in wounding your adversary, and seek to complete your work by recovering the spearhead and allowing it to rust away, in order that he may simultaneously languish and die, you are assuming this time that the intimate association between weapon and wound has set up so much identity between two situations that their future developments must to a large extent be the same.

It seems highly probable that beliefs of this character arose as interpretations of observed facts, and it is most unlikely that they have survived through ages without the

* *Op. cit.*, i, pp. 61, 62.

† *Cf.* Bradley, *Principles of Logic*, p. 133.

‡ *Op. cit.*, i, Ch. II, esp. pp. 10–18 and 56 *et seq.*

support of facts which have been taken to be verifications of them; there must, at least, have been a widespread *belief* that they "worked." *Formally*, then, the processes are unexceptionable, and differ from a modern investigation apparently only in the *material* circumstance that now-a-days we should not fix upon these particular "cores of identity" in the situations contemplated as having any relevance to the similarity between the courses of their subsequent development alleged to be observed. Since, however, mistaken beliefs as to the significance of certain elements of phenomena have been common in the history of Science, if we are to find an *essential* difference between Science and Magic we must look elsewhere.

We can find the *differentia* we are seeking only by considering the whole primitive attitude towards the Objective, the system of beliefs and interests by which new phenomena were "apperceived." The primitive thinker had not reached the clear distinctions we make between the dead world and our living and conscious selves, and peopled the physical environment with active individual principles whose wills had constantly to be reckoned with. Moreover, his attitude towards this environment was determined to a predominant extent by considerations that touched the immediate safety and wellbeing of himself and of his tribe. To a very large extent it was the attitude of a being who combined with the passions and vices of a man the terror of the child in the presence of the unknown. Bearing these two facts in mind, the failure to distinguish between the animate and the inanimate which made him regard the environment as a great community of beings, for the most part to be dreaded or placated, and the constant pressure of the needs of defence and preservation, which made it necessary that *something should be done*, we can understand his at first sight capricious logic, and can see the psychological force of the considerations which led ultimately to his submission to the burden of a

rigid system of beliefs and customary acts " heavy as frost and deep almost as life."

§ 16.

It was such a system of interpretations of the Objective which was losing its authority at the momentous epoch which we mark as that of the birth of Greek Philosophy. Philosophy, the child of Wonder, began when advancing knowledge was banishing the nymph and dryad from the world of practical activity to the fantasy world of the poet, when no longer the Ionian could

" Have sight of Proteus rising from the sea ;
Or hear old Triton blow his wreathèd horn."

With the realisation of the inadequacy of the once sufficing explanations of the world's happenings, there arose the need for more satisfactory ones, while the widening and deepening of intellectual interests that came with an age of comparative personal and social security, brought men face to face with the old problems of change and decay in a much more general form. The motive of the movement, which we commonly date from the speculations of Thales, was to seek escape from the intellectual oppression of the world's ceaseless flux in some abiding reality. The animistic "moment" was passed, but men had not yet come to that realisation of the great gulf fixed between their real selves and physical nature which is the distinguishing mark of the modern consciousness.* We find accordingly that the new effort to render the Objective intelligible takes the form of an attempt "to give back to Nature the life of which it had been robbed by advancing knowledge . . . . simply by making it possible for that life which had hitherto been supposed to reside in *each* thing, to be transferred to the one thing of which all others were passing forms." † Animism was replaced by *Hylozoism*.

* Martineau, *Types of Ethical Theory*, i, pp. 123, 124.
† Burnet, *Early Greek Philosophers*, p. 13.

Once more we have to distinguish the "secular conative process" here initiated from Science. That the Greeks collected material indispensable to the structure of Science is not to be disputed, whatever estimate we adopt of the actual value of their achievements on the whole and in detail. As a result of recent research that estimate has undoubtedly tended to rise.* We can no longer accuse them of an entire neglect of physical experiment, and the late Professor Huxley, after a careful consideration of the existing records, arrived at "a very favourable estimate of the oldest anatomical investigations among" them.† Burnet has, moreover, defended the hastiness with which hypotheses were advanced upon the warrant of a very slender bridge of facts, regarding this haste as naturally characteristic of early undisciplined enthusiasm, and retorting effectively that the same fault is by no means absent from the history of modern investigation.‡ Finally, Jowett has attributed to these "general notions" a positive value, regarding them as "necessary to the apprehension of particular facts . . . Before men can observe the world, they must be able to conceive it." §

Against these apologies it must be maintained that, with certain exceptions that hardly affect the argument, the "scientific" achievements of the Greek thinkers were simply incidental to the search for the "abiding reality" which is the predominant characteristic of the whole intellectual movement. This which was true of Milesian Nature-philosophy, was still more obviously true when their speculations gave place to the "moralised" conceptual investigations in Being and Becoming of Heraclitus and his Eleatic opponents. We must maintain the same of Empedocles, though he "anticipated" the theory

---

* *See* Mach, *Science of Mechanics*, 2nd Eng. ed., App. I.

† *On certain Errors attributed to Aristotle* in *Science and Culture*, p. 193.

‡ Burnet, *op. cit.*, p. 26.

§ Introduction to the *Timaeus: Dialogues*, iv, p. 416.

of organic evolution, though his *ῥιζώματα* were the direct ancestors of the modern elements, and though his "mechanical" *Weltbildung*, in which, besides these *στοιχεῖα*, only the forces of Love and Hate play their part, may not so fancifully be compared with the object of physical science as conceived (for example) by Helmholtz.* To be brief, not even the elaborate systems of Democritus and Aristotle can be exempted from the general statement that we are dealing here with attempts to render the Objective intelligible which, on the ground of an essential difference in the whole "situation," must be distinguished from Science.† To justify this statement fully would obviously require so much time that I must ask to be forgiven for stating dogmatically a contention the principle of which you will, I hope, be inclined to admit without further argument.

§ 17.

For the same reason it is impossible to do more than illustrate the fact that my contention also holds good of many modern thinkers, who have yet made contributions to the fabric of Science of fundamental importance. In the case of these moderns the individual systems of ideas by which Objective facts were apperceived were dominated by theological as well as philosophical elements. Thus Descartes when, to complete his philosophical system, he turns his attention to the actual particulars of the behaviour of the *res extensa*, deduces (in an imperfect form) the modern doctrine of the Conservation of Momentum from considerations of the perfection of God! ‡ A

* *Ueber die Erhaltung der Kraft*, Einleitung, p. 6: "Die Naturerscheinungen zurückzuführen auf [Materie und] unveränderliche, anziehende und abstossende Kräfte."

† *Cf.* Plato's view that "the movements of the stars are only bad diagrams illustrating the truths of ideal astronomy," or Aristotle's conception of laws valid only "ἐπὶ τὸ πολύ," with Galileo's conviction that unbiassed investigation of matter will explain all apparent anomalies in its behaviour. [*Dialogues*, Weston's trans., p. 3.]

‡ Descartes, *Principia Philosophiae*, 2nd part, § 36.

little later Leibniz corrects the deficiencies of this principle, pointing out that Descartes had neglected to observe that the direction as well as the quantity of "force" (momentum) is conserved. Our interest fastens on his further remark that if Descartes had noticed the fact, "he would have fallen into my System of Pre-established Harmony."*

§ 18.

But for the illustrations most suitable to my purpose I must direct your attention to the writings of Keppler; for the student who picks his way discreetly through Frisch's monumental edition† of the *Omnia Opera* of that heroic astronomer, will gain as his reward a vivid idea of how profoundly the whole "situation" in which Objective facts are actually central is determined by the character of what I have already called the "embroidery"; and will, moreover, catch sight of the human spirit at the precise moment of one of its most interesting metamorphoses.

Keppler begins (in the *Mysterium Cosmographicum*, 1596) as an enthusiastic young convert to the heliocentric doctrine of Copernicus. He defends the new theory on the ground of its superior simplicity, not, *bien entendu*, its simplicity as a *description* of the facts, but its *real* and meritorious simplicity as an actual creative plan.

"Amat [Natura] simplicitatem: amat unitatem. Nunquam in ipsa quicquam otiosum aut superfluum extitit; at saepius una res multis ab illa destinatur effectibus."‡ One form of orbit, then, should be *expected* to suffice for all the planets, instead of the deplorably diverse orbits of the Ptolemaic system. In the spirit thus indicated he proceeds to determine the reasons why the solar system could not but be precisely as it is. First we learn why a combination of curves and linear distances (from

* Leibniz, *Monodologie*, § 80; *Théodicée*, § 61.
† Keppler, *Omnia Opera*, ed. Frisch, Frankfort, 1858–71.
‡ Cap. I, p. 113 (Vol. I. of Frisch's ed.).

the Sun) should be exhibited: "Quantitatem autem Deus ideo ante omnia existere voluit, ut esset curvi ad rectum comparatio."* Moreover, these curves will lie upon spherical surfaces so as to exemplify the Trinity: "[Imago] Patris scilicet in centro, Filii in superficie, Spiritus in aequalitate σχεσεως inter punctum et ambitum."†

Similarly there must have been the best of reasons for the choice of the particular dimensions of the orbits, the general principle being, "Nefas est . . . . quicquam nisi pulcherrimum facere eum qui esset optimus."‡ So it was inevitable that the Creator should lay the foundations of the planetary worlds in accordance with the ideas He would gather from His contemplation of the Five Perfect Solid Figures. Imagine the sphere of which the circular orbit of Saturn is a central section, to be circumscribed about a cube, then the sphere which contains in a similar manner the orbit of Jupiter will be inscribed within this cube. Next, within the sphere of Jupiter let a regular tetrahedron be inscribed; this will in turn circumscribe the sphere of Mars. Thus we reach all the planets in turn, finding it obvious that Man—finis et mundi et omnis creationis—should have his habitation in the midst of the planetary host, three celestial bodies guarding his path without, three (including the Sun) within.§

So far you will agree, the course of Keppler's investigation has exemplified my remark that, *formally*, non-scientific attempts to render the Objective intelligible may not differ from those which are admittedly scientific. We have the usual primary Objective basis and the usual secondary construction—the Objective facts qualified by an "hypothesis." But the secondary construction here exhibited (you will object) is one that is capable of *verification—i.e.*, of predicting new

* Cap. II, p. 122.
† *Ibid.*
‡ *Ibid.*
§ Cap. IV, p. 128.

Objective facts which contributed nothing to the determination of that construction. The relative distances of the planets from the sun in Keppler's system are open to calculation and comparison with *data* of observation. Keppler's theological prepossessions do not prevent him from recognising this truth in the clearest manner. "Transeamus modo," he says, "ad *ἀποσηματα* orbium astronomiae et demonstrationes geometricos: quae nisi consentiant, procul dubio omnem praecedentem operam luserimus."* So the relative radii of the spheres imprisoned in this complicated way between the regular solids are computed, and the results compared with the estimates of Copernicus. The concordance is practically perfect! †

In 1600 Keppler left his chair at Gratz, and received from Tycho Brahe that introduction to the Emperor Rudolph which led to consequences of the first importance in the development of Science. Brahe died in the same year, and the Imperial mathematician inherited his splendid collection of observations on the planet Mars. In 1609 appears the famous treatise, *De Motibus Stellae Martis*, in which he sets forth with the delightful long-windedness of a leisurely age the results of his patient study of these *data*. After the fashion of a day when philosophers reasoned even of Ethics *more geometrico*, Keppler prefixes to his work a collection of *Axiomata physica de motibus stellarum*. These are of the highest interest, for they betray a complete change (since the *Mysterium Cosmographicum*) in the astronomer's attitude towards his facts. To determine the particulars of the orbits of the planets we are no longer invited to consider that they must move "ad majorem Dei gloriam: motus a spatio dependet; planetae aguntur vi naturali; vis motrix opus habet propagatum a fonte ceu effuxu"; are among the startling "axioms" that meet us.

The body of the work is largely occupied by Keppler's

---

* Cap. XIII, p. 148.

† Table in Cap. XIV, p. 151.

famous demonstration that the orbit of Mars instead of being a circle, as the prepossession in favour of "perfection" had hitherto compelled him to suppose, is actually an ellipse of which the sun occupies one focus. It will interest us more to attend to the remarkable change in his whole attitude towards the Objective upon which I have already remarked. We find the evidences of this change most prominent in the introduction and the later chapters of the treatise. *Ce n'est que le premier pas qui coûte,* and when Keppler has once been compelled to seek the secondary construction that is to make the primary facts intelligible in a disinterested study of those facts themselves in their quantitative determination, he travels fast towards a characteristically "modern" point of view. Since the planets no longer move in circles they must resign with these the crystal spheres in which since the days of Plato they have been "quiring to the young-eyed cherubim." These destroyed, what is to guide a planet's motion? The *anima mundi* remains, it is true, and Keppler, like his great contemporary Gilbert, finds nothing objectionable in the conception. He had, in fact, used the admitted existence of the *anima mundi* as an argument against the Ptolemaic orbits, inviting his readers to pity the condition of the distracted world-souls who in that complicated system "ad tam multa respicere jubentur ut planetam duobus permixtis motibus invehant!"* Similar considerations seem to deter Keppler from assigning to the *anima mundi* the perpetual solution of the mathematical difficulties incidental to following an elliptical path round an eccentric sun. He looks elsewhere for a means of, at least, lightening the world-soul's burden and finds what he wants *within the Objective itself* in a new conception of the sun as *fons motus.* This conception has not been reached without external suggestion, and when we meet the phrase *orbs virtutis tractoria* we are left in no doubt as to the source of that

* *Introd.*, p. 149 (Vol. 3 of Frisch's ed.).

suggestion. Keppler has been reading the newly-published treatise *De Magnete*, by Gilbert of Colchester, the Father of Experimental Science, and has fastened upon the fruitful analogy between magnetic and stellar phenomena. The first result is a "true doctrine of gravity" which points directly to the completer doctrine of Newton. Repeating the argument given above in connexion with the *anima mundi* Keppler asserts the impossibility "ut forma lapidis movendo corpus suum quaerat punctum mathematicum aut mundi medium."* On the contrary, "gravitas est affectio corporea mutua inter cognata corpora ad unitionem seu conjunctionem (*quo rerum ordine est facultas magnetica*) ut multo magis Terra trahat lapidem quam lapis petit Terram."† There are, it is true, difficulties in the application of the analogy. The investigations of a Galileo were necessary before a Newton could see that the moon is actually and always falling towards the earth. For Keppler the difficulty is to account for their remaining apart: "Si Luna et Terra non retinerentur *vi animali aut alia aliqua aequipollenti* . . . Terra ascenderet ad Lunam . . . Luna descenderet ad Terram . . . ibique jungerentur."‡

The words italicised in this passage illustrate at once Keppler's willingness to retain the conception of the *anima mundi* and his growing preference for a *facultas corporea* to a *facultas animalis* if the former can make the facts intelligible. We may leave the consideration of the development of his ideas at the point where he reaches a "secondary construction" of the facts of the stellar observations suggested altogether by such material analogies. In this conception the planetary movements are ascribed to a two-fold "virtue"—one of the planet and one of the sun. That of the planet is compared with the work of oars in rowing, that of the sun to the stream of the river. And so we reach the all-important conclusion, in which the soundness of this conception is based upon the

* *Introd.*, p. 150. † *Introd.*, p. 151. ‡ *Ibid.*

solid experimental results of Gilbert: "Quale flumen, talis remus. Flumen est species immateriata virtutis in Sole magneticae. Quin igitur et remus de magnete quippiam habeat? Quid si ergo corpora planetarum omnia sunt ingentes quidam rotundi magnetes? De Terra (uno ex planetis, Copernico) non est dubium. Probavit id Gulielmus Gilbertus."*

It will be noted that Keppler's final conception of the planetary system is *formally* less satisfactory than the earlier one—since it fails to suggest quantitative determinations by which it could be verified. At the same time it will, I hope, be agreed that when, at some moment between 1600 and 1609, Keppler, wrestling with Brahe's records, forgot his pious prepossessions in his anxiety to understand the behaviour of Mars *for the sake of understanding it*, he adopted for the first time an attitude which was genuinely "scientific." The *differentia* of Science, then, as a conative process whose aim is to render the Objective intelligible, is the presence of no motive except the *desire* to render it intelligible—particularly in its quantitative determinations. No philosophical leanings, not even the desire of power over Nature for which Bacon was willing to be her minister, can be admitted beyond the "margin" of the apperceptive area in which the Objective facts are central. The scientific attitude is essentially that of the *savants* who, drinking to the next great discovery, coupled with their toast the hope that it might never be of any use to anybody.

§ 19.

I need hardly say that Keppler does not provide us with the first example on record of the scientific attitude. Mach holds that the beginnings of Science are to be found in the descriptive communications of the processes of the craft made by older members of a guild to beginners.† So Höffding,‡

* Pars Quarta, Cap. LVII, p. 387.

† Mach, *Science of Mechanics*, p. 4.

‡ Höffding, *Hist. of Modern Philosophy*, i, p. 161.

with truth, says that "the appearance of a Leonardo or a Galileo* is only comprehensible when taken in connexion with Italian industry." But industrial pursuits, I suggest, can never do more than supply the *experience* which forms the starting point for the scientific process which follows only from a specific attitude towards that experience which I have tried already to characterise. Just as J. A. Symonds has shown us that in the epoch of the Crusades and dominant Scholasticism the Latin songs of the Wandering Students "gave clear and artistic utterance" to a "bold, fresh, natural, and pagan view of human life"; so, doubtless, ever and anon men of intellect turned aside from the theologico-philosophical studies of their day to the task of rendering intelligible Objective facts in which they took an immediate interest and delight. Such a one, in part, was Roger Bacon, such a one was his master, Peter of Maricourt,† such a one pre-eminently was Leonardo da Vinci who, though his discoveries do not appear actually to have affected the course of Science, left among his remarkable manuscripts a presentment of the scientific attitude which can hardly be improved. I conclude this section by quoting a typical expression of his opinion:‡ "In dealing with a scientific problem I first arrange several experiments, since my purpose is to determine the problem in accordance with experience and then to show why the bodies are compelled so to act. That is the method which must be followed in all researches upon the phenomena of Nature. It is true that Nature as it

---

* *Cf.* the opening words of Galileo's own Dialogues: "The constant employments in your famous arsenal of Venice, and especially those relating to what we call Mechanics, seem to me to afford, to a speculative genius, a large field to philosophise in." (Tr. Weston.)

† To whom Gilbert of Colchester was much indebted See in Bridges' edition of Bacon's *Opus Majus*, 1897–1900, vol. i, p. xxv.

‡ From Grothe, *Leonardo da Vinci also Ingenieur und Philosoph*, Berlin, 1874, p. 22. *Cf.* the following passage: "Le me pare che quelle scienze sieno vane e piene di errori, le quali non sono nati dall' experienza, madre di ogni certezza, e chi non terminano in nota experienza." *Frammenti litterari e filosophici*, p. 94.

were begins with reasoning and ends with experience, but nevertheless, *we* must begin with experience, and by means of it strive after the discovery of Truth."

"The interpreter of the wonders of Nature is experience. . . . We must consult experience in the variety of cases and circumstances until we can draw from them a general rule that is contained in them. And for what purpose are these rules good? They lead us to further investigations of Nature and to creations of art. They prevent us from deceiving ourselves or others by promising results to ourselves which are not to be obtained."

CHAPTER III.

§ 20.

A "natural history" of the more sustained attempts that humanity has made to render the Objective intelligible—that is to give it a place in a definite apperceptive system—would lead us, then, to the conclusion that its *differentia* is not, as has been frequently supposed, a peculiar method, but simply and solely a definite attitude of the "Self of the moment" towards the Objective, a definite character of the system by which new elements are "apperceived," a character only to be expressed by saying that this system is dominated by a permanent interest in the particulars of the Objective as such. The next chapter in our natural history would examine in turn the various special attempts to make the Objective intelligible which are included in the genus "scientific." Such an examination would, I submit, bring out the fact that it is difficult to declare any concept essentially incapable of mediating a scientific interpretation of the Objective to some thinker. Thus it has already been pointed out that Keppler in his "scientific" period did not shrink from continuing to utilise the conception of the *anima mundi.* A less violent but essentially similar example is the use of the concept of *cause* in the sense of transeunt action—a notion with which some scientific thinkers have entirely dispensed, while to others it is of cardinal importance. Facts of the same order are the marked preference of Weber and his Continental school for the concept of action at a distance and the equally marked preference of the British school for the concept of an intervening medium as a means of rendering action at a distance intelligible. Especially illuminating in this connection are the well-known facts that Maxwell based his immensely important electro-magnetic

theory upon the concept of a "displacement" to which it is impossible to assign a definite meaning,* while Lord Kelvin, speaking on the same subject said, "As long as I cannot make a mechanical model all the way through, I cannot understand, and that is why I cannot get the electro-magnetic theory of light."† An almost better illustration is afforded by Dr. McDougall, who not only conceives his "neurin" as a fluid, but defends his practice in an excellent note‡ by arguments essentially the same as those I am advancing. Finally, it is clear, that this doctrine of the relation of the scientific concept to the primary facts does not exclude the concept of "end" from the investigator's armoury of interpretative weapons and so admits the methodological propriety of the practice of "neo-vitalists" such as Bunge and Rindefleisch. Our doctrine, moreover, has a normative value. It declares that a concept which is to render given primary facts intelligible must be formed as a reaction upon the stimulus of the presentation of those facts in their actual determinations. While it admits, then, the aid of any concept borrowed from any other context, it refuses to allow Objective facts to be annexed simply in order to widen the territories of an aggressive theory, and still less to permit their *primâ facie* deliverances to be ignored through a bias in favour of any particular type of interpretation. Thus "electricity" and "neurin" may both be legitimately conceived as fluids, but the physicist is not to rule the concept of "interaction" or of a "soul"§ out of court, and still less is he to refuse to entertain evidence in favour of "telepathy."||

The only restriction upon the secondary construction is that its form shall be determined by the actual particulars of the

---

* See Merz, *Op. cit.*, ii, 93.

† Quoted by Ward, *Naturalism and Agnosticism*, i, p. 119.

‡ McDougall, "The Physiological Factors of the Attention Process," *Mind*, N.S., No. 43, p. 350.

§ *Cf.* James, *Pr. of Psych.*, i, p. 137; McDougall, *Physiological Psychology*, pp. 8 *et seq.*, p. 78.

|| As at least one very distinguished scientist is reported to have done.

primary facts. This condition limits the usefulness of such a conception as the *anima mundi* or the "end" to a phase in the development of knowledge of the facts when the particulars are not capable of full determination.* At such a time such a concept as vitalism may legitimately be used "as a comfortable halting place where the reason may be laid to rest on a pillow of obscure ideas" when there is "danger of premature and, therefore, inadequate physico-chemical explanations of the phenomena of life."†

§ 21.

It is in almost identical words that Bunge opens the important work in which he treats of physiological chemistry from a vitalistic as opposed to a "mechanistic" point of view.‡ At the same time he avows that his adoption of this position is due to his definite dissent from "the doctrine which some opponents of vitalism maintain, and which would have us believe that in living beings there are no other factors at work than simply the forces and matter of inorganic nature."§ In opposition to this doctrine, Bunge denies that physico-chemical explanations have been given of any really *vital* phenomena—*i.e.*, of any phenomena in which what we call "activity" is displayed. Thus even in so simple a case as the absorption of food from the alimentary canal, the physical processes of diffusion and osmosis do not account for the established facts. The epithelial cells exercise a selective function, accepting globules of fat, but declining (for instance)

* Thus Bunge (*Text Book of Physiological and Pathological Chemistry*, p. 420) explains the high temperature in fever "teleologically" as an attempt of the organism to destroy the bacteria. But this explanation fails to yield the precise temperature reached.

† Prof. Hering, quoted by McDougall, *loc. cit.*

‡ Bunge, *Text Book of Physiological and Pathological Chemistry*; translated from the 4th ed. by Starling, 1902, p. 1: "No explanation is offered by a mere term. I regard 'vital force' as a convenient resting-place where, to quote Kant, 'reason can repose on a pillow of obscure qualities.'"

§ *Loc. cit.*

fine pigments.* Similarly, *Vampyrella Spirogyræ*, only a minute unicellular mass of protoplasm, insists on feeding upon the alga, *Spirogyra*, and rejects every substitute.† Still more remarkable is the "behaviour" of *Arcella*, a unicellular organism protected by a concavo-convex shell which on the concave side is pierced by a hole through which pseudopodia are pushed out. Arcella insists that these pseudopodia shall be attached to something solid, and to secure this end generates bubbles of gas so as to raise one edge of its shell, or, if necessary, to turn itself completely over.‡

The last example challenges comparison with the behaviour of the Slipper-animalcule, *Paramecium*, which has been studied and interpreted by Dr. H. S. Jennings.§ The Paramecia "feed largely upon clotted masses of bacteria. If a number are placed upon a glass slip, together with a small bacterial clot, they will be sure to congregate around the clot and to feed upon it. All apparently press in so as to reach it or get as near as possible." Moreover, they appear generally to be "actuated by some social impulse leading them to crowd together and shun isolated positions." They display, again, definite likes and dislikes towards the various liquids that may be introduced into the water in which they are swimming; "from alkaline liquids they are repelled; to slightly acid drops they are attracted, unless the acidity be too pungent."

Dr. Jennings' interpretation of these performances is antithetical to Bunge's explanation of the behaviour of Arcella. It consists in an attempt to reduce the phenomena to cases of "organic response" which are conceived as of the same type as the orderly reactions of inorganic matter. The Paramecia

* *Op. cit.*, p. 3. Similarly Professor Japp claimed that the mould *Penicillium* "selects" the right-handed variety of racemic acid, *B.A. Report*, 1898, p. 817.

† P. 3.

‡ P. 7.

§ I quote partly from the account given by Lloyd Morgan, *Animal Behaviour*, 1900, Ch. I.

crowd round the bacterial clot if, and only if, in the course of their random movements they are brought in contact with it. This mere contact produces an automatic "lessening or cessation of the regular movements in all the cilia except those of the mouth-groove and funnel." In a similar spirit Dr. Jennings deals with "the seemingly social assemblages of Paramecia, where there is no such substance to arrest their progress," showing that this phenomenon is nothing more than a special case of the "attraction" which feebly acid solutions possess for the animalculæ. Where one or two Paramecia are gathered together, they form a centre for the production of a feebly acid solution of carbon dioxide, which is, as with all creatures, a product of their organic waste; "and when other Paramecia come, in the course of their random movements, into such a centre they remain there and help to swell the number in the cluster." After a time the solution in the centre of the group becomes so strong as to be repellent instead of attractive. "The group spreads, and the Paramecia are less densely packed." Finally, to meet the objection that in this attraction and repulsion "at least there is, it may be urged, an element of choice," Dr. Jennings once more takes pains to attribute the phenomena to a definite "reaction" which the cilia of the Paramecia undergo when they reach the boundary of the acid drop.

The general conclusion which he reaches is, in fact, that "the reactions of Paramecium are, as we have seen, comparable in all essentials with those of an isolated muscle [of a frog]"*—and, therefore, that "we are not compelled to assume consciousness or intelligence in any form to explain its activities."†

§ 22.

Before leaving the consideration of these specimens of antithetical interpretations of the same kind of *data* we should

* *Amer. J. of Psych.*, x, p. 515. † *Ibid.*

not fail to note that they are the expression of definite diverse methodological views held by the two naturalists. Thus, Bunge* holds that "the essence of vitalism does not lie in being content with a term and abandoning reflection, but in adopting the only right path of obtaining knowledge which is possible, in starting from what we know, the internal world, to explain what we do not know, the external world."

On the other hand, Dr. Jennings† proceeds in obedience to the canon that such a "complex psychology as seems forced upon us by the observed facts" should be "reduced to simple factors" as far as possible, and, in the course of his study of the problems presented by the Paramecia, makes it more than probable that, in his view, a factor is "simpler" the more closely it approximates to the reactions of physics and chemistry. There can be no doubt that at the least he would subscribe assent to the cautious words in which Professor Lloyd Morgan, at the end of his description of Dr. Jennings' experiments, points a moral upon which he has consistently acted throughout the whole of his own well-known and valuable researches:—"One of the first lessons which the study of animal behaviour in its organic aspect should impress upon our minds is that living cells may react to stimuli in a manner which we perceive to be subservient to a biological end, and yet react without conscious purpose—that is to say, automatically. . . . If purpose there be, it lies deeper than its protoplasm, deeper than the dim sentience which may be present or may be absent—we cannot tell which."‡

§ 23.

It seems a fair critical summary of this controversy to say that it gives us a concrete example of the principle that facts lying within a new province of experience may be made

* *Op. cit.*, p. 10.
† *Amer. J. of Psych.*, x, p. 506.
‡ *Animal Behaviour*, p. 10.

intelligible by means of concepts drawn from diverse fields of experience already explored; the choice of concept being determined only partly by the actual Objective data, and for the other part by psychological conditions.* Of course, it may be maintained that there is really a question of *facts* at issue: a *purpose* either does or does not lie at the back of the phenomena. Here we are confronted by a difficulty which will be considered at a later stage.† We may decline to discuss it in the present connection on the ground that few thinkers, if any, would regard the Objectivity of a purpose as capable of proof except as a "residual phenomenon" after the application of all possible physico-chemical explanations. It is not likely that any biologists would maintain that this stage has actually been reached. We may conclude then that the only considerations which should interfere with the purely individual *choice* of one type of interpretation over another are considerations, if such can be arrived at, of the relative usefulness of particular kinds of hypotheses at the particular stage of exploration of the province of fact under discussion.‡

§ 24.

From this point of view the opposition between "causal" and "final" explanations appears in a new light. Both "cause" ("*Ursache*") and "end" or "purpose" ("*Zweck*") are concepts abstracted from definite experiential contexts. We have maintained that any such concept may be used to make Objective facts intelligible to an individual investigator: the question as to the *Objectivity* of the cause or the purpose may ultimately have to be decided in a given case, but at the

---

* It is not implied, of course, that the *selection* of the Objective *data* for interpretation is independent of these psychological conditions. Bunge and Dr. Jennings, starting from different points of view, inevitably deal with *data* of somewhat different characters. *See* p. 144.

† *Infra*, Ch. V.

‡ *See*, for example, Bieganski, *Neo-Vitalismus in der moderne Biologie* (*Ann. der Naturphilosophie*, IV, i).

outset, at any rate, is irrelevant. Since the same question may arise with regard to other concepts which are used to make a certain group of facts intelligible (*e.g.*, with regard to the Objectivity of the "ether" or of "atoms") it is clear that causal and final explanations are to be regarded as on the same level with an indefinite number of others. This being the case it seems hardly worth while to distinguish these particular types of interpretation by a special terminology which simply keeps alive the memory of a controversy which, from the point of view adopted here, has vanished.

Nevertheless, these traditional terms may very usefully be retained with somewhat new implications.* Thus, without intending any suggestion of "transeunt action" we may use the term "causal law" as a convenient name for statements which exhibit the dependence of the behaviour of a thing upon external conditions. In this sense it is a causal law that two "material particles" in one another's presence, but infinitely remote from all other matter, would exhibit accelerations of their velocities, directed along the straight line joining them, inversely proportional to their masses and to the square of the distance between them. The statement of *any* such a "causal law" may be called a "causal explanation" of the behaviour of the particles in question, being usefully distinguished by this term from explanation by an "empirical law" which would simply give a formula for the behaviour of a single particle without reference to the behaviour of any other body. But, as a matter of fact, scientific investigators always endeavour to bring the observed behaviour of bodies under some *definite* concept or concepts. Thus, in Mechanics, it is sought to describe the behaviour of bodies by means of the concept of the Conservation of Energy, or of Force (Conservation of Momentum), or of Least Action, or one of a number of

* The first formal suggestion of this use with which I am acquainted is in an interesting article by Ostwald (jun.) and Blossfeldt, *Ueber Kausale und finale Erklärung* (*Ann. der Naturphilosophie*, III, 1, pp. 111 *et seq.*).

subsidiary descriptive notions such as Harmonic Vibration. Similarly, the chemist seeks to bring other Objective happenings under the concept of Equivalence, while the biologist will employ in a similar manner the notion of Natural Selection or of Panmixia. These attempts to bring the particulars of experience under such *special* concepts may one and all be called "final explanations"; the purpose which makes that term appropriate being, of course, the conscious endeavour of the investigator to apply this special form of solution to the problems before him. On this view, then, all the special interpretations of the sciences are final explanations; while all are to be regarded as subordinated to the general notion of causal explanation, in so far as they aim at exhibiting the dependence of the behaviour of things upon external conditions.

In conclusion, attention should be directed to the fact that we are here concerned only to maintain the right of every investigator to choose his own weapons wherewith to reduce to intelligibility the primary facts within the province of the Objective which he has marked out for conquest. Upon the subject of the choice at any particular stage in any of the special sciences each such science must be "self-normative." This contention does not forbid us to hold, with Professor Boyce Gibson,* that, on the whole, "the essential discovery of modern science is that its ideal, the thorough understanding of nature, can be reached only by subordinating the idea of end entirely to that of law," nor yet to agree with Lotze† in feeling "certain of being on the right track, when [we] seek in that which *should* be the ground of that which *is*."‡

* *A Philosophical Introduction to Ethics*, p. 53.

† *Metaphysics*, ii, p. 319.

‡ I much regret that the extremely interesting little book on *The Interpretation of Nature*, by Professor Lloyd Morgan, did not fall into my hands until after the above discussion had been written. I may, perhaps, be allowed to refer the reader for a fuller discussion of the origin and import of the causal concept to my paper "On Causal Explanation" in the *Proceedings of the Aristotelian Society* for 1906–7.

§ 25.

I am aware that in view of the vigorous and important attack upon "hypotheses" made by writers of such eminence as Ostwald* my defence of them will appear reactionary. I venture to think, however, that Ostwald fails to distinguish between the *real* value and the *psychological* value of hypotheses. Hypotheses, such as Maxwell's displacement, the weight of a molecule, electrons, the carbon-tetrahedron, entropy, heat itself, may not be *verifiable* and, therefore, have no real value, but their psychological value as "leading us to further investigations of Nature" and prompting to fresh determinations of the Objective may be immense. Ostwald's assertion that scientific advance has taken place in spite of, and not by means of, hypothesis† is, at best, a half truth. It is true that hypotheses have temporarily delayed the progress of Science in some particular field, but when they have disappeared they have generally been devoured by their own children—objective determinations to which they led. To maintain that these determinations would have been made without the hypotheses—for example that Maxwell, without the concept of electro-magnetic displacements in the field around a varying current would have thought of locating at points in the field the disembodied relations expressed by his differential equations, the manipulation of which led Hertz to discoveries of the highest importance, seems itself to be an indulgence in hypothesis of a thoroughly unwarrantable character. The point of Ostwald's objection to a hypothesis—a *Bild*‡ used to make the phenomena intelligible —is that the *Bild* will invariably contain elements which are not present in the original observations. There are two answers to this objection. In the first place it may be urged that this

* Ostwald, *Vorlesungen über Naturphilosophie*, X, esp. pp. 211–215.

† Ostwald, *op. cit.*, p. 225.

‡ "Dass . . . . man durch die Benutzung des Bildes in die Darstellung der Erscheinung Bestandtheile hineinbringt, die dem Bilde angehören, nicht aber der Erscheinung selbst," *op. cit.*, p. 212.

property of the hypothesis is that which above all makes it valuable. The portion of the Objective under investigation must be the seat of other relations than those "apperceived" by the concept, and it is already probable that the original analogy will extend to the other properties of the concept whose correspondence with properties of the Objective under examination has not yet been established. Thus "a descriptive theory of this kind does more than serve as a vehicle for the clear expression of well-known results, it often renders important services by suggesting the possibility of the existence of new phenomena."*

In the second place, physicists are so sensible of the aid they receive from such a descriptive hypothesis, that they do not discard it even when it is recognised as containing elements actually inconsistent with known Objective determinations. The conception of the ether as a frictionless fluid passing among the molecules of matter "more freely than the wind through a grove of trees," has been none the less useful because incompatible with the rigidity which the facts also seem to demand. Ultimately, of course, such incompatibility will not be tolerated, but its very presence sets a further problem—the replacement of the inconsistent hypotheses, both having reference to the same province of Objective fact, by another which shall do justice at once to all its elements. Such a complete correspondence between the elements of the descriptive hypothesis and of the province of the Objective is, of course, the ideal of the scientific process, to which the successive concepts by which it is sought to render the facts intelligible approach, as Mach says, "asymptotically."† Were it attained the "picture" and the "object" would coincide‡ and we should have "a

* Prof. J. J. Thomson, introducing his conception of the "Faraday tube" as an alternative to Maxwell's "displacement." *Recent Researches in Electricity and Magnetism*, 1893, p. 1.

† Mach, *Principien der Wärmelehre*, 1900, p. 461.

‡ "Wenn Bild und Gegenstand in allen Stücken übereinstimmten, so

complete systematised representation," "a complete synoptic [*übersichtliches*] inventory of the facts of the province" of the Objective free from the extraneous elements that hypothesis admittedly introduces.* When this consummation has been reached in any department of Science, descriptive hypotheses will still have a psychological value for the purposes of exposition and assimilation. Meanwhile they will continue to play an indispensable part in the conquest of the Objective, whether in definite form as Lord Kelvin's "mechanical model all through," or a vague form like Maxwell's "displacement," being, as it were, *points de repère* without which great systems of reasoning cannot be built, just as transient ones require the aid of shadowy visual, auditory or kinæsthetic images.

Finally it may be pointed out that it is of small consequence to the progress of the special sciences whether the investigator attaches real value to his hypothesis, or whether he recognises that it is merely psychological. Lord Kelvin and Principal Rücker are quoted by Dr. Ward† as examples of the former class, holding that in the ether and in atoms and molecules we have realities behind the veil of phenomena, while Maxwell in his attitude towards his earlier model of the ether,‡ Wollaston, Davy, Liebig and Faraday in their attitude towards Dalton's atoms, are given by Dr. Merz§ as examples of the second. It seems probable that in the case of the latter class of investigators their attitude towards their conception is

---

wären sie eben dasselbe, d. h. man kann eine Erscheinung vollkommen nur durch sich selbst abbilden." Ostwald, *op. cit.*, p. 212.

* Mach, *loc. cit.*

† Ward, *op. cit.*, i, pp. 113 and 306.

‡ "I do not bring it forward as a mode of connection existing in nature. . . . . It is, however, a mode of connection which is mechanically conceivable and easily investigated . . . . so that I venture to say that any one who understands the provisional and temporary character of this hypothesis, will find himself rather helped than hindered by it in his search after the true interpretation of the phenomena." *Collected Papers*, i, p. 486 ; quoted by Merz, *op. cit.*, ii, p. 83.

§ Merz, *op. cit.*, i, p. 418.

rhythmic, at one time yielding to a temporary belief in them, at another time criticising them as from an external point of view.

§ 26.

These properties of the hypothesis may all be well illustrated by a brief study of the part played in the development of the science of Chemistry by the concept of the atom. I have already expressed my acceptance of the view that the real significance of the atom in the thought of the Greek school of atomists was its metaphysical significance—its value in connection with the solution of the problem of reconciling the Eleatic and Heraclitian views of the universe. The atom had had a respectably long philosophical history before it appeared in the speculations of Democritus, but there can be no doubt that its origin was the ordinary hard body of well-defined shape and size of everyday experience. In taking this concept from its accustomed context, and using it to "explain" the whole of the structure and behaviour of the world, Democritus neglected those qualitative differences between things which the *ὁμοιομέρειαι* of Anaxagoras had preserved;* but he retained the shape and size and the definite modes of movement which characterise the molar body as we know it, and built up his whole system on the basis of their assumed explanatory value. In this connection it is interesting to note that Bacon, who preferred Democritus to the idealist philosophers of antiquity, was well aware that an atomic theory must necessarily be an attempt to explain some of the phenomena of matter by means of others. Accordingly he twits Democritus with being "sibi impar et fere contrarius"† not merely for retaining in a theoretical construction which is intended to explain *all* properties of things, some properties unexplained, but also actually using these retained properties as the means of explanation of

* *See* Gompertz (Eng. trans.), *Greek Thinkers*, i, p. 331.

† *De Principiis atque Originibus: Works;* ed. Ellis and Spedding, iii, p. 82.

the rest. Bacon's criticism is, as a matter of fact, only directed upon the assumption of definite modes of motion for the atoms,* which modes are admittedly drawn from observation of bodies on the earth's surface, but the spirit of his objection is the same as that which I have expressed to the notion that hypotheses have a real as distinguished from a psychological value.

§ 27.

It is well known that Bacon's general approval of the methods and views of the ancient atomists was part of the positive aspect of his rejection of the Aristotelian scholastic system of philosophy, and that his own method of Forms did not show that he had found in his study of the neglected work of the laughing philosopher the clue that would lead men out of the labyrinth of their ignorance of the world about them. Nor did the Lord Chancellor, who "wrote on Science like a Lord Chancellor," perform the service of pointing out to others the way to the Promised Land which he was himself not qualified by temperament to enter. That function fell to the lot of Gassendi, who, turning to the atomic philosophy as he found it expressed in the luscious hexameters of Lucretius, "not by accident, nor out of mere love of opposition . . . . embraced with a sure glance exactly what was best suited to modern times and to the empirical tendency of his age. Atomism, by *his* means drawn again from antiquity, attained a lasting importance, however much it was gradually modified as it passed through the hands of later inquirers."†

The interests of Gassendi, like those of his friend, our own Thomas Hobbes, were in problems of a philosophical as opposed to a scientific character; but in an age when the notion of a division of intellectual labour had not yet arisen, and men still essayed the task of encompassing a knowledge of the answers

* "Debuit enim motum heterogeneum atomo tribuere, non minus quam corpus heterogeneum et virtutem heterogeneam." *Loc. cit.*

† Lange, *History of Materialism*, Eng. trans., 1877, i, p. 255.

that their fellows have given or are giving to all the problems of Time and Existence, it was inevitable that this revival of atomism in the fields of speculation should attract the attention of the two great men who were destined to be most prominent in naturalising the philosophy of Gassendi and Hobbes in the positive sciences.*

§ 28.

To Robert Boyle belongs the credit (in addition to his most notable discoveries in Physics) of applying the new corpuscular notions to rescue the investigation of the chemical properties of matter from the unworthy hands of the alchemists In his dialogues entitled *The Sceptical Chymist,* he conducts (through the mouth of the character Carneades) a polemic against the "Peripatetick Doctrine," in which, carrying a step further the revolt initiated by "Paracelsus and some few other sooty Empiriks,"† he propounds in a perfectly clear form the modern concepts of chemical compounds and elements. "It seems not absurd to conceive that at the first Production of mixt Bodies, the Universal Matter whereof they among other Parts of the Universe consisted, was actually divided into little Particles of several sizes and shapes variously moved."‡ "Neither is it [im]possible," continues Carneades, "that of these minute Particles divers of the smallest and neighbouring ones were here and there associated into minute Masses or Clusters, and did by their Coalitions constitute great store of such little primary Concretions or Masses as were not easily dissipable into such Particles as composed them."§

It is clear from the discussion that follows, as well as from the more systematic exposition given in another work|| that the

* Lange, *op. cit.*, i, p. 300.

† I quote from the English edition, Oxford, 1680.

‡ P. 37.

§ P. 38.

|| Boyle, *The Origines of Formes and Qualities* (*According to the Corpuscular Philosophy*). I quote from the second edition, Oxford, 1667.

"primary Particles," "*Minima* or *Prima Naturalia*"* are analogous to the "ions" of the "one Universal Matter" from which are built up the "atoms" of the elements to which Boyle's "primary Concretions" correspond, while the "molecules" of the modern chemist are anticipated by the "grosser and more compounded Corpuscles"† that compose such bodies as the "exotick Compounds" already mentioned.‡

Mercury (to take one of Boyle's examples) "will with divers Metals compose an *Amalgam*, with divers *Menstruums* it seems to be turned into a liquor, with *Aqua fortis* it will be brought into either a red or white Powder or precipitate, with Oyl of Vitriol into a pale yellow one, with Sulphur it will compose a blood-red and volatile Cinaber, with some Saline Bodies it will ascend in form of a salt which will be dissoluble in water. . . . . And yet out of all these exotick Compounds, we may recover the very same running Mercury that was the main ingredient of them, and was so disguised in them."§

In this way a concept drawn from the context of metaphysical speculation served as the instrument which enabled Boyle to render a definite group of primary facts intelligible. The outcome of its application in a few cases was that a definite new *problem* emerged into view: the problem of showing that of the manifold substances that compose the material universe the majority can be regarded as compounds of a few simple elements which may be thought of as existing side by side in the compounds, disguised but not destroyed. When the corpuscular hypothesis had led to the formulation of this permanent problem for the chemist face to face with a new substance, it had, for the time being, accomplished its task, and ceased to be any longer an instrument of investigation in chemistry.

---

* *Origines of Formes*, p. 47.
† *Ibid.*, p. 48.
‡ *The Sceptical Chymist*, p. 41.
§ P. 40.

§ 29.

Meanwhile, however, a vastly more powerful intellect than that of Boyle was preparing the atom for a wider career of usefulness. Newton's *Principia* made it possible for Laplace a hundred years later to present to Napoleon the atomic Weltanschauung in which he had found no need of any such hypothesis as that of a Creator. In an admirable chapter on "The Astronomical View of Nature," Dr. Merz has drawn a most lively and interesting picture of the way in which Newton's astronomical views "spread into molar and molecular physics," and has traced this enormously fertile stream of investigation and discovery to its sources in the notion of attraction (to which for the first time Newton gave a quantitative meaning), and in Newton's own suggestion made in the famous "Query 31" with which the "Optics" closes, that the behaviour of material substances might in a multitude of cases be interpreted by means of the notion of attractive or repulsive forces acting between the hard, solid particles of which they might be supposed in ultimate analysis to consist.

§ 30.

But it was not until ten years ago, when the twelve volumes of John Dalton's lecture and laboratory notes were discovered in the rooms of the Literary and Philosophical Society of Manchester, where his experimental work was carried out,* that it became more than a shrewd biographer's surmise that the father of modern chemical theory had drawn his inspiration from the same source. At the conclusion of the notes on a course of twenty lectures delivered at the Royal Institution† during the winter of 1809–10, Dalton copied

* Roscoe and Harden, *New View of Dalton's Atomic Theory*, 1896, p. 12.

† P. 125.

several extracts from Query 31, and also the enunciation of the 23rd Proposition of Book II of the *Principia*, to which in the 17th lecture he had made a definite reference. From Query 31 Dalton doubtless drew the general conviction that "the Changes of corporeal Things are to be placed only in various Separations and new Associations and Motions of these permanent Particles";* and, in particular, laid hold of Newton's opinion that "God in the Beginning form'd matter in solid, massy, hard, impenetrable, moveable Particles, of such Sizes and Figures, and with such other Properties, and in such Proportion to Space, as most conduced to the End for which he form'd them."† The stress that Dalton lays upon this assertion of the possibility of the existence of "particles of matter of several sizes and figures, and in several proportions to the space they occupy, and perhaps of different densities and forces,"‡ is due to his preoccupation with the problem of accounting for the fact that "the oxygen gas being specifically heaviest should not form a distinct *stratum* of air at the bottom of the atmosphere and the azotic gas one at the top of the atmosphere."§ Before "modern discoveries [had] ascertained that the atmosphere contains three or more elastic fluids, of different specific gravities,"|| the difficulty had not arisen. Newton had shown that "particles flying each other with forces that are reciprocally proportional to the distances of their centres, compose an elastic fluid whose density is as the compression"¶—that is which follows a law that Boyle had shewn to be true of air. Moreover, it was possible to suppose that the repulsion necessary to make the solid atoms of a single

* Newton, *Opticks*, p. 376.

† *Opticks*, p. 375.

‡ This passage is given by Roscoe and Harden (p. 125) as a further quotation from Query 31. It appears rather to be a note of Dalton's own upon the passage printed above.

§ Roscoe and Harden, *op. cit.*, p. 14.

|| P. 13.

¶ Newton's *Principia* (Motte's trans., 1729), ii, p. 77.

gas "fly each other" could be supplied by surrounding them with a relatively bulky elastic atmosphere of heat.* But Dalton's problem was to understand how uniform diffusion could occur in a mixture of atoms of different densities; and it proved to be impossible to bring about this result by adjusting their atmospheres of heat, without contradicting known facts about the specific gravities of the gases involved.† Then it occurred to Dalton to make the particles of his oxygen, azote, and water vapour of different sizes (meaning by *size* "the hard particles at the centre and the atmosphere of heat taken together"). If this were done, he supposed that (as in a mixture of shot of two or three different sizes) the particles of one size would not be able to form a system in equilibrium with particles of another size, but would ignore their presence; equilibrium being reached only when the different types of particles had become equally diffused. But there was an apparent obstacle to this view. "At the time" when Dalton "formed the theory of mixed gases" he "had a confused idea, as many have, that the particles of elastic fluids are all of the same size; that a given volume of oxygenous gas contains just as many particles as the same volume of hydrogenous."‡

Fortunately, perhaps, for the history of chemistry, Dalton saw reason to reject§ a doctrine which later became equally with his own ideas part of the fundamental basis of chemical theory. This obstacle removed, he believed that his theory of gaseous interdiffusion held the field, and at once "it became an object to determine the relative *sizes* and *weights*, together with the relative *number* of atoms in a given volume. . . . Other bodies besides elastic fluids, namely, liquids and solids, were subject to investigation, in consequence of their combining with elastic fluids. Thus a train of investigation was laid for determining

* Roscoe and Harden, *op. cit.*, p. 19.

† Pp. 14, 15.

‡ Dalton, *A New System of Chemical Philosophy*, 1808, p. 188.

§ Dalton, *op. cit.*, p. 71.

the *number* and *weight* of all chemical elementary principles which enter into any sort of combination one with another."*

From these most interesting notes it appears that the origin of Dalton's Atomic Theory was not an attempt to explain the fact that combination may occur between the same two substances in more than one proportion,† but an attempt to render intelligible a group of physical phenomena. In the course of this attempt, Dalton turned for *data* of the relative weights of his atoms to the determination of the composition of different substances that had been made by contemporaries of repute; being already, from his physical considerations, convinced that chemical combination takes place between particles of different weights. "The extension of this idea to substances in general necessarily led him to the law of combination in multiple proportions,"‡ and thus, as the result of this renewed application of the concept of the atom, a new definite problem was presented to chemists which has guided their researches ever since; a problem which would generally be stated as that of determining what *atoms* of the elements enter into the composition of the "compound atom" or molecule of every compound.

§ 31.

In the face of these historical facts it seems somewhat paradoxical to maintain, as Professor Divers does in his Presidential Address to the Chemical Section of the British Association,§ that the atomic theory propounded by Dalton "is not founded upon the metaphysical conception of material discontinuity, and is not explained or illuminated by it."‖

* Roscoe and Harden, *op. cit.*, p. 17.

† This common belief is due to Thomson, *History of Chemistry*, i, p. 80, ii, p. 291.

‡ Roscoe and Harden, *op. cit.*, p. 51.

§ *B.A. Reports*, 1902, pp. 557–75. *Cf.* the criticism of Divers' views by Meldrum, *Avogadro and Dalton*, 1904.

‖ P. 558.

But a study of this valuable and interesting address shows that Professor Divers' real concern is to propound and illustrate a doctrine of the *functions* of the atomic hypothesis *quâ* hypothesis which is identical with that defended in this essay. For him, as for us, the starting point of the science of chemistry is a definite series of Objective facts. "The facts of a chemical nature about common salt which cause the statement to be made that it is a chemical compound of chlorine and sodium are such as these:—Salt can be wholly changed into sodium and chlorine; these substances brought together change into salt and nothing else; salt and sodium, each under conditions appropriate to it, change into the same substance, called also a sodium compound, such as sodium hydroxide; salt and chlorine, each in its own way, change into the same chlorine compound, such as hydrochloric acid; neither sodium nor chlorine, one apart from the other or the other's chemical compounds, ever changes into salt . . . ." Some such "primary facts" as these, presenting themselves in a few instances to Robert Boyle, were apperceived, that is, were rendered intelligible to him by the concept of undissolved "primary concretions" of ultimate hard particles of different kinds existing side by side in the "grosser corpuscles," and recoverable therefrom without change. This concept, as we saw, led to the formulation of a definite problem—the problem of determining in the case of a given substance what other substances can "combine" to form it: or, as Professor Divers (following Professor Duhem in his "return to Aristòtle"*) has taught us to say, what substances "change" into it and under what conditions. Quite similarly, the really important result from a critical point of view of the renewed application by Dalton of the atomic concept to chemical facts, is that it has led to the definite formulation of a further question. This is the problem of determining what weights of substances display

* Duhem, *Le Mixte et la Combinaison chimique*, Ch. I.

a new kind of equivalence to one another, which we may express by saying that they are "chemically equal,"* just as by saying that two bodies have "equal mass" we imply that they display a definite kind of dynamical equivalence to one another.† The atom—a concept which entered into modern science in the seventeenth century from a philosophic context in which its function was to harmonize the contradictions of Being and Becoming—thus comes to be "the qualitative and quantitative expression of the powers possessed by substances to change into others,"‡ an expression which contains no more than an historical allusion to the view that "matter" is composed of actual minute indivisible particles.§ In possession of the concept of a definite Objective relation constantly showing itself between definite Objective facts—the facts of chemical combination—we have, indeed, reached the point at which at least one particular group of elements of the "picture" and the "object" coincide; || and we have no further use, except, possibly, for purposes of exposition and the like, of the earlier concept which first enabled us to bring intelligibility into this region of Objective facts.¶

§ 32.

Whenever in the foregoing allusion has been made to the fully determined particulars of a province of the Objective, it is highly probable that the reader will have assumed that quantitative or at least numerical determinations were intended. It is a commonplace that Science only moves with security where she can measure. Quite recently we have seen this

---

* Divers, *op. cit.*, pp. 559 and 563.

† *Vide infra*, p. 99; Divers, *op. cit.*, p. 562.

‡ Divers, *op. cit.*, p. 566.

§ P. 559.

|| *Supra*, p. 72.

¶ *Cf.* an article "On Methods of Teaching the Atomic Theory," contributed by the writer to *The School World*, February, 1906.

truth demonstrated anew in the field of Biology, where Professor Karl Pearson* has so brilliantly illustrated old Roger Bacon's dictum that Mathematics is the "gateway and key to *all* other Sciences"; while, doubtless, even before Egyptian priests began to survey the lands left dry after the inundations of the Nile, men felt the application of number and measure to the spatial world to be natural and obvious.

But as Mr. Russell has shown, if A is 12 inches and B 24 inches from O, there is really an element of convention in the familiar assertion that B's distance from O is twice as great as A's.† Those distances are definite relations which cannot strictly be identified with the relation of one number to another. The fuller truth is that it is possible, since the numbers form a "continuous series," to correlate every position on the straight line O B with a single number, while there is a practical convenience in arranging the "one-one correlation" in such a way that if the distance (*i.e.*, the spatial relation itself before the advent of measurement) between O and A is equal to that between A and B, the difference between the numbers assigned to O and B is twice the difference between those assigned to A and O.

By the simple device of measuring with the foot rule, we are able to overcome the difficulty that different perceived distances between A and B have yet the same "representative value,"‡ that is, refer to the same real distance. Much the same holds good of such conceptions as temperature and weight. The same body at the same time may be pronounced by two different persons to be hot and cold, a result which is taken to mean not that the thing *is* both hot and cold, but that the felt hotness and coldness are simply different representatives of the same objective value. If a thermometer is placed in

---

* Pearson, *Phil. Trans.*, vol. 185 *et seq.*, also *The Grammar of Science*, 2nd ed.

† *Op. cit.*, p. 180; also *supra*, p. 29.

‡ Stout, *Proc. Arist. Soc.*, 1903, *loc. cit.*

contact with the body it is taken for granted that the different positions of the surface of the mercury are each correlated with one objective condition of the body. Thus if the thermometer gives the same reading in the wind as it does behind a screen, then the air, although it feels colder in the open, must really be in the same objective condition, have, as Boyle vividly expresses it, the same *temper*, in both places.* If now we "graduate" the stem of the thermometer upon the foot-rule method, we shall have a series of numbers correlated with the various "tempers" or temperatures of the body. In this case the statement that one difference of temperature is double another has obviously still more of the conventional character than we noted in the case of distances,† for we have no method of deciding that the difference between temperatures A and B is equal to the difference in the case of B and C comparable with the use of the foot-rule in spatial measurement or of the pendulum in time determination.

§ 33.

The relation of this "objective condition" to what we have hitherto described as "elements of the Objective" obviously demands discussion. It will at once be noted that there is no reason to suppose that the "real" hotness of the body is ever presented to any of the observers; at any rate, it is certain that it is never presented with any certificate of its supremacy to the presentations of hotness which other observers experience. We have found ourselves, in fact, compelled to regard *all* such presentations as standing on the same level of Objectivity‡ and have been driven in consequence into a certain amount of metaphysical system-building. It is clearly opposed to the

* This problem is discussed by Boyle in his *Experimental History of Cold*, 1665, First Discourse; also p. 513.

† Kelvin's "absolute thermometric scale" seeks to avoid this conventionality. A brief account of the theory of this scale of temperatures is given in Ch. IV, *infra*, § 52.

‡ *Supra*, p. 15.

spirit of the whole of our previous argument to admit at this point the Objectivity, in our own sense, of a concept which is quite obviously an hypothesis imported—like other hypotheses which we have seen and have yet to see—into the undoubtedly Objective *data* simply in order to render them intelligible. Yet at the same time an hypothesis which is instinctively adopted by everyone to whom the *data* are presented is something so very much like the Objective form of synthesis that constitutes other Objective elements into a "thing"* that its case seems to have a right to special consideration.

It would appear that justice is done to its claims by the admission that it is a *postulate* which has reference, strictly, not to truth, but to action. It is true that in the sense explained in § 5 a thing may not absurdly be said to have at the same place and the same time many not-identical hotnesses, but for practical—especially for social—purposes the recognition that this is so would be highly inconvenient. For such purposes the richness and many coloured variety of the actual primary facts must be replaced by an artificial simplicity; a result which is reached by the instinctive hypothesis of a "same representative value" which is attributed to the diverse Objective presentations. The innumerable hotnesses perceived around the body are conceived to represent, or to have reference to, a "state of the body itself" which, since it is independent of perception, is truly the same for all. This postulate—which we may call the postulate of "pragmatic objectivity"—performs then, a function somewhat analogous to that of the Kantian categories; it makes certain kinds of social activity possible which without it could hardly take place.

It is possible that we have here a not unhappy example of many cases in which a distinction may profitably be made

---

* *Supra*, p. 17.

between what is "pragmatically" and what is Objectively true; between elements which are "the same for all" because they are "relevant to purpose" and not because they are Objective; and elements whose sameness for all and relevance to purpose are co-ordinate consequences of their Objectivity. It is, I would further surmise, to "pragmatic truth" that the maxim "Simplex sigillum veri" properly applies. Since Lotze uttered his protest against its erection into a universal logical principle* the progress of science has done much to make more and more doubtful Keppler's early opinion that "amat Natura simplicitatem";† while it has made the conceptual simplification of the *data* a more and more essential condition of theoretic success.‡

To return to the case under consideration, we may make one or two observations which throw light upon the readiness with which the suggestion of "a same representative value" is accepted. As we have seen, the commonsense view of a "thing" must, when subjected to philosophical rectification, include the notion of an indefinite number of hotnesses which are bound in one nexus of Objective relations. These hotnesses undoubtedly have an identical reference to something, namely, to the "thing" or whole composed of qualities and relational nexus to which they belong—or, rather, to what may be called the cross-section of this whole at the moment of observation. It is not difficult to understand that by the plain man, whose keen interest in the practical efficiency of an idea is unequally yoked with a very poorly developed disposition to criticism of its philosophical adequacy, they come to be thought of as having reference to an identical *hotness*, much as the qualities of individual Englishmen are thought of as having reference to a "typical Englishman." Finally, the acceptance of the recurrence of the thermometer index to the same point

* *Logic*, ii, p. 88.

† Poincaré, *Science et Hypothèse*, p. 173.

‡ *See* Larmor, *B.A. Report*, 1900, pp. 617–8.

as a sign that the same state of hotness has recurred presents little difficulty. It is simply a *fact* that while many hotnesses may be simultaneously perceived around the place which a body " occupies " in the narrower sense of the term, presentations such as the one in question do not appear in such bewildering multiplicity. Thus, the position of the index which is believed to be actually the same for all is readily correlated with the postulated real state of the body.

Later, when attempts are made to fix with greater accuracy the position of the index, the old difficulty breaks out anew. The assumed uniqueness of the determination vanishes; for it is found that no observer is consistent with other observers, nor even with himself. This time, moreover, it is not possible to appeal from the fluctuating presentations to a stable one which can be assumed to be correlated with the Objective value which the former fail to indicate unambiguously. We are confined to the circle of the actual discordant observations, and, if we are to reach this real Objective value at all, must place our hopes entirely upon some fortunate manipulation of our *data*. As is well known, the mathematical theory of observations indicates a method by which we may calculate the "probable error" of the series of determinations in question—that is, a method which enables us to determine a number which represents a deviation from the arithmetical mean of the observations within which it is an " even chance " that the object of our search—the real position of the index—will fall.

It would be difficult to decide the true interpretation of this result. It is, of course, possible that the different observers actually have the same presentations but fail to distinguish them with accuracy—the error (of which we do not claim to be able to give a rational account)* following the law assumed in the mathematical theory. On the other hand, it is, perhaps, permissible to suggest that when the observations are simply

---

* *Cf.* § 6 (last paragraph).

records of diverse presentations that have occurred to the same or to different observers under identical external conditions, their diversity cannot properly be compared with the results of imperfect muscular or instrumental adjustments such as those which produce (to use an illustration much favoured in this connection) the distribution of hits around the bull's-eye of a target. What we have maintained to be true of the diverse perceived hotnesses may be true of the diverse perceived positions of an index. They may have reference not to a "real" position of the index which the actual perceptions aim at with only imperfect success, but to an Objective relational nexus which contains them. The fact that these presentations form a series between the extreme terms of which there is (in the terminology of § 8) only a small distance, may render the ordinary assumption of the theory of errors "pragmatically" valid; just as Hooke's Law is pragmatically valid for small deformations of any solid body.

In conclusion attention should be drawn to the fact that in the view defended in this section and in the first chapter, the various perceptions of hotness round a body are not regarded as "effects" of a thing-in-itself lying beyond perception. There are Objective relations of implication between the several hotnesses at a given moment of time, and in this sense alone can it be admitted that "the principle of causality underlies the whole procedure"* by which changes of hotness at a given place are regarded as due to changes in the thing itself—this "thing itself" being no more than the whole body of perceived qualities *plus* the nexus of Objective relations by which they are connected. This interpretation of the facts seems capable of satisfying the demand of common-sense for a "thing" that shall be more than a mere bundle of "possibilities of sensation"; while at the same time it avoids the serious difficulties that ensue when a thing-in-itself, inaccessible to observation, is made the

---

* Stout, *Proc. Arist. Soc.*, N.S., IV, p. 145.

"cause" of the series of actually presented facts. Nevertheless this interpretation is by no means incompatible with the belief that things have an "inner being"—possibly "ultimately psychical," like the inner being of another person's toothache* —so long as this inner being is not regarded as the reality of which the facts accessible to observation are merely appearances.

§ 34.

When a hot body is placed near colder ones it gets colder, they get hotter. These primary facts become intelligible—are systematised—by the thought of a transference of "something" from the one to the other. This something is *heat.* Black,† who made such important conquests for Science by means of this concept, was one of those who are able to keep on their guard against the dangers which Ostwald sees in the *Bild.* He declines to form any definite conception of the relation of the heat to the substance which occupies the same space, on the ground that no Objective facts are before him to justify his doing so. But if heat is regarded as a substance at all, the "amount" of it which reaches the cold bodies must be thought of as equal to that which left the hot body. The problem is set therefore of finding "something constant" at both ends, so to speak, of the transaction. If a steady flame is the "source of heat," it is impossible not to suppose that the "quantity of heat" leaving the flame per minute is always the same. Let us place above the flame in succession different weights of water each for the same length of time. Examination of the results shows that the product of the weight of water by the rise of temperature is in each case the same. This constant product, then, may be identified with the "quantity of heat" of which we are in search.

This simple example will serve to illustrate the weighty

---

* Stout, *op. cit.*, p. 158.

† Black, *Lectures on Chemistry*, 1803.

remark, made so long ago as 1867 by Rankine,* that "one of the chief objects of mathematical physics is to ascertain, by the help of experiment and observation, what physical quantities are 'conserved.'"

The illustration also brings out the fact that the constancies established in such investigations are of an entirely conventional character and refer to nothing Objectively "transferred." We *assumed* that the two temperature changes were different aspects of the same transaction, an assumption whose consequences are made psychologically available by throwing it into the form of a transfer of "heat." We correlated the various terms of the series of temperatures and weights which appear in this transaction with numbers. If our initial assumption was correct, it seems now that *some* manipulation of the *data*—here the weights and temperature changes—*must* yield an equality, the particular form of this manipulation depending upon the particular manner in which the number series has been correlated with the series of objective states of the body. Our success in finding the desired manipulation implies that, in the language of Lotze,† the bodies *do* "take note" of one another's changes of condition, and that the *data* we have manipulated, that is the original *data* with which numbers were correlated, are the complete expression of that "notice." In short, it is the verification in a particular case of the postulate of the rationality of the world.

§ 35.

On the other hand, failure to find this manipulation will always prompt the investigator to look further afield to discover other modifications of things which must be considered as elements, hitherto unrecognised, of the transaction in question. Thus, to pursue our illustration a little further,

* Quoted by Merz, *op. cit.*, p. 140. *See also* Divers, in *B.A. Report*, 1902, pp. 557, *et seq.*

† Lotze, *Metaphysics* (Eng. Trans.), i, § 45, p. 118.

if a definite "amount of heat" is "given" repeatedly to a mass of air whose volume in one experiment is kept constant and in other experiments is made to vary in an arbitrary manner, it will be impossible to find a manipulation of the mass and temperature changes of the air which will yield a constant number that can be equated to the "quantity of heat given." In other words, the change of temperature of the air is, in this case, not a *complete* "notice" of the change in the body which acted as the "source" of the heat. Some other partial expression or expressions of that notice must be sought, and in this case will be found, in the "work done" by or upon the gas during the experiment. As is well known, it was the perception of the significance in this and other cases of the "work done" that led Mayer, Joule, and (later) Helmholtz to the enunciation of the essentially modern conception of "energy."

§ 36.

The result of such a process as has been described in the foregoing section is always to bring the bodies between which the new relations have been established into some sort of unity. This unity will take various forms and display various degrees of permanence, to attempt a classification of which would be scarcely profitable. But it is interesting to note how frequently the "notice" taken by one body of another's behaviour is thought of in the form of a transference of substance. Besides heat and energy, momentum, electricity, magnetism, and other cases will suggest themselves. This transference sometimes is thought of in a shape which implies that one part of the system is gaining what another part is losing. Such is the case of the transference of heat which we have just considered. In other cases the transference takes a "circulatory" form as in the case of the "current" of electricity. In this case the various bodies concerned—wire, battery, galvanometer needle, &c.—are thought of as forming one

compound "thing" which displays many of the properties of the genuine "things" of common sense. Thus the perception of a certain deflection of a galvanometer needle may be regarded as the *perception* of a certain "current," just as, when a certain noise falls upon a man's ear, he asserts that he "hears" a hansom cab.*

§ 37.

We have now reached, perhaps, a point from which we obtain a clearer view of the circumstances under which, in the history of Science, psychical events came to be excluded from the causal series. To suppose that they are legally banished under the terms of Hume's famous edict against investigations that do not "contain any experimental reasoning concerning matter of fact and existence," is a view that no one could hold "except to save a theory."† And if they suffer through the condemnation pronounced against inquiries that do not "contain any abstract reasoning concerning quantity or number" we see that this defect is not essential to their nature as events, for "series" prevail in the psychical as widely as in the physical world. The difficulty is reduced to the practical difficulty of establishing for the terms of these series (which, as I have pointed out in the first chapter, are regarded as being "the same for all") an unambiguous correlation with the terms of the equally Objective number series which happens, like much of the physical, to be not only the same for all, but also accessible to all. Were such a correlation established it would apparently be possible to determine whether certain psychical changes and physical changes are or are not complete expressions of the "notice" which soul takes of body or body of soul.

---

* *Cf.* Poincaré, *La Valeur de la Science*, p. 227.

† Bradley, *Appearance and Reality*, p. 324.

CHAPTER IV.

§ 38.

The special characteristic of the foregoing doctrine is its insistence upon the co-ordinate Objectivity of many different types of experience and the paramount importance of respecting this Objectivity and of regarding the particulars of experience in which it is *prima facie* present as the ultimate facts. These may, indeed, be shown to be related to other elements of the Objective, previously unknown, or not known to have a special relation to those in question, but may never be thought of as "explained" (in the sense of being explained away) by the discovery of these relations. At the same time our doctrine does not forbid the scientist to build upon the basis of these primary facts secondary constructions in which elements from other categories of experience are involved. The alien elements thus introduced often serve the important function of bringing to light relations between the primary facts which could hardly have been discovered without their aid. This truth has been illustrated sufficiently in the last chapter. The secondary construction can, without difficulty, be conceived to be extended so as to unify the primary facts both in the province from which the interpretative elements are borrowed and the province in which they are applied. In this way a result may be achieved which, for the purposes of scientific investigation and exposition, may be regarded as a *reduction* of the facts of the latter province to facts of the former. It is obvious that the extent to which unification of the various provinces of scientific inquiry can thus be brought about is identical with the range over which a concept, or a group of concepts, drawn from a single experiential context, can be applied to make facts intelligible.

It is well known that physicists have repeatedly attempted a reduction of the happenings of the physical universe in terms of the concepts of mass and motion. This chapter will be devoted to a brief analysis of the main features of these attempts.

§ 39.

We have seen that the investigators of the seventeenth and eighteenth centuries had reached clear concepts of the *temperature* of a body as its real state of hotness or coldness, and of *heat* as an hypothetical substance whose transference from one body to another renders intelligible the changes of temperature that occur when bodies are brought near to one another. Black, to whom this concept in its modern form is due, has the credit of applying it to phenomena—many of which he himself discovered—which before his consideration had not received adequate theoretical explanation. The most important of these were the phenomena attending the transformation of a substance from the solid to the liquid, or from the liquid to the gaseous condition. If (for example)* a pot of water is heated above a steady flame its temperature will not rise indefinitely. When the "boiling point" has been reached the whole of the water will be converted gradually into steam—the most interesting fact (unknown before Black's researches) being that the temperature of the steam is no higher than that of the water. The constant and fairly regular rise of temperature before the boiling was attributed to a regular transference of heat from the flame to the water. It would be contradictory to suppose that this transference ceased when the water began to boil. Consequently, since the heat-substance is not "sensible" in the form of an increased temperature of the water or steam, it must have entered into a special combination with the particles of the water—a

* Black, *Treatise on Chemistry*, 1803, i, p. 164.

combination that accounts for the change of state which the water undergoes: it becomes "latent heat."*

§ 40.

But the concept which Black had handled with such splendid audacity was not destined to a long career of theoretical adequacy—although for elementary didactic purposes it is still indispensable.† Within fifty years of Black's researches the experiments of Romford and Davy had made it clear that heat must be thought of as capable of being generated without limit by mechanical work.

The effect of this discovery may be represented symbolically by the statement that it was now impossible any longer to conceive heat as a substance; critically, it implied that the changes of temperature of bodies cannot be regarded as a closed series of events; but that a change of temperature may, in fact, sometimes be the way in which a body "takes note" of a change of the "mechanical" order in another body. At this point, therefore, it becomes necessary to submit the fundamental notions of mechanics to a brief critical scrutiny.

§ 41.

The development of these ideas presents, in many respects, an interesting parallel with the development of the ideas of the science of Heat.‡ We begin with a number of kinæsthetic and "pressure" sensations whose object is conceived vaguely as the "force" of the body to which they are due—the heavy stone, the stretched string, the bent rod, the moving missile. It is recognised (as in the case of hotness and coldness) that these sensations, even when they are different, may often refer to the same

* Black, *op. cit.*, pp. 176–7.

† *See* a lecture by the present writer printed in the *Educational Times* for May, 1905.

‡ I have considered it from this point of view in an article on "'Mass' and 'Force' in School Mathematics" in *School* (December, 1905) The next few paragraphs are in the main a condensation of that article.

objective* "force." Thus, though the force required to stretch a given elastic cord, or to bend a given rod to a definite extent, will seem very different when one is fatigued and when one is fresh, very different in the cases of a man and a child, we admit instinctively that it must "really" be the same in every case where the same change has been brought about by manipulating the cord or the rod in the same manner. Such changes in the cord or rod would readily be regarded, like the positions of the index of the thermometer, as objective records of the forces applied, and would need only to be correlated with numbers by means of an arbitrary scale (such as the divisions of a foot rule arranged suitably in connection with the cord or the rod) to be practically useful. In whatever way the change in the cord or the rod has been brought about it is always possible to find a body whose weight applied in the same manner as the force in question can produce the same change in length or shape, so that we pass readily to the practice of equating forces to the weights of bodies. When this stage is reached it is possible by a very natural convention of a character previously discussed† to proceed beyond the mere *recording* of forces to their *measurement:* a force equal to the weight of 2 lbs. is thought of as twice a force equal to the weight of 1 lb.

§ 42.

At this stage it is clear that a different method must be used for estimating the "force" which is ejectively thought of as present in a moving body; and the method just described loses its theoretical security even for static measurements when it is discovered that the weight of the same body is different at different points of the earth's surface.‡ The need is now

* In the sense explained on p. 86.

† *Supra*, p. 28.

‡ Vaguer notions of the distinction between the mass and the weight of a body arose historically from simpler experiences (*cf.* Maxwell, *Theory of Heat*, 9th ed., p. 86).

felt of another concept to replace the weight of a body as a measure of what has vaguely been thought of as the "quantity of matter" in it. Examined critically, this need is found to resolve itself into an instinctive demand for some constant coefficient that shall characterize the behaviour of the same body in all "dynamical transactions"* into which it enters, including those in which it presents the property of a variable weight. Investigations similar to those which Huygens and his English contemporaries carried out upon colliding bodies† supply the best materials for a clear answer to the demand thus expressed, and lead at the same time to a perfectly general and satisfactory concept of force. If two inelastic bodies moving directly towards one another with equal velocities of any given value bring one another to rest, it is found that however their velocities are changed in amount,

---

* Clerk Maxwell's happy phrase.

† No. 43 of the *Philosophical Transactions* (January 11, 1668–9) contains an enunciation of Dr. Wallis's *General Laws of Motion* (pp. 864–6), and of Wren's *Lex Naturæ de Collisione corporum* (pp. 867–8). Wallis dealt with inelastic bodies; Wren (like Huygens) with ideally elastic bodies. In No. 46 (April 12, 1669) *Regulæ de Motu Corporum ex mutuo impulsu*, "communicated by Mr. Christian Huygens in a letter to the Royal Society" were published, like the communications of Wallis and Wren, "in the language of the learned" for the benefit of Continental as well as of English inquirers. In connection with the hypothetical course of investigation followed in the text it is interesting to note that Wren had formed a concept of "velocitates corporum propriæ et maxime naturales [quæ] sunt ad corpora reciproce proportionales." The property of such velocities is that "[duo] corpora, R et S, habentia proprias velocitates, etiam post impulsum retinent proprias;" while a general law (for elastic bodies) is expressed in the form, "Quantum R superat et S deficit a propria velocitate ante impulsum, tantum ex impulsu abstrahitur ab R et additur ipsi S, et e contra."

Huygens, on the other hand, reaches, in Regula 8 (p. 928) the general law of the text in the form "Quantitas motus [*i.e.*, momentum] duorum corporum augeri minuive potest per eorum occursum; et semper ibi remanet eadem quantitas versus eandum partem, ablatâ inde quantitate motus contrarii."

It is instructive to notice, also, that Wren uses for the modern "mass" the unexplained term "corpus," while Huygens explicitly states of his bodies that "eorum moles æstimatur ex pondere."

so long as they remain equal the same result will follow. Consequently these bodies may be thought of as possessing a certain equivalence to one another which we may express by saying that they have *equal mass.* Directly or indirectly it may be shown that bodies of equal mass as thus defined have also, at the same place of observation, equal weight. Since we have agreed to the convention that a body whose weight is equal to the weight of two other bodies of equal weight shall be considered as having twice the weight of one of them, it is natural to think of such a body as having at the same time twice the mass of one of them. But the circumstances under which the term "equal mass" became applied to the original pair of bodies do not suggest directly any sense which the assertion "A's mass is twice B's" can bear; so that experiments must be instituted to discover transactions in which it will be possible to find a useful *meaning* for the assertion in question that shall be in accordance with the assumption that mass is directly proportional to weight. This meaning is reached when it is found that if we take two inelastic bodies A and B of which A is n times as heavy as B and therefore has, *ex hypothesi,* n times the mass, these bodies will bring one another to rest if they collide with velocities inversely proportional to these masses. As the result of such observations the "mass" of a body becomes definitely conceived as the reciprocal of the ratio which the velocity of the body in question must bear to that of a standard body whose mass is taken as unity, if in inelastic collision the two bodies are to bring one another to rest.

§ 43.

We have now to reach the conviction that this coefficient characterises the behaviour of the body in other transactions besides collisions. Before this step is taken we must note that the equivalence which we have described as equality of mass is only a special case of the more general equivalence pre-

sented by bodies so circumstanced that the product of mass into velocity is the same for each. To this equivalence is given the name, equality of *momentum*. Thus the momentum which Descartes thought of as a measure of the "force" of a moving body is properly a piece of descriptive apparatus by means of which an account may readily be given of cases in which colliding bodies bring one another to rest. By an easy extension of the experiments to cases in which rest does not result, it is found that this descriptive instrument has a still wider range of usefulness. Recognising that momentum is a "vector quantity,"* we may bring *all* cases of collision under one formula: "The total momentum of the two bodies is the same after as it was before the impact; momentum being merely transferred from one body to the other."

But experience presents us with cases in which two bodies enter into transactions with one another which involve a gradual change of momentum instead of the sensibly instantaneous transference which characterises impact. In certain cases (*e.g.*, when two bodies of different mass are drawn together by an elastic cord stretched between them) it can be demonstrated that the movements of the bodies under these new conditions can still be brought under the old descriptive formula: momentum is not lost during the transaction but is constantly being transferred from one body to the other. But in this case of gradual transference the question of the *rate* of transference at a given moment is one that is bound to arise;

* *I.e.*, a quantity which is completely specified only when its direction and "sense," as well as its magnitude, are given. Such quantities can be represented by *vectors*, that is by straight lines of proportional length, drawn in the appropriate directions, the "sense" being indicated by an arrow-head on the line. Vector quantities, must, moreover, follow the law of *vector addition;* in other words, two or more of them must be replaceable by a single one, which is represented by the "sum" of the vectors of the others. The "sum" of the vectors, α, β, γ, ..., is the vector obtained by drawing α, β, γ . . . . end to end (the arrows all pointing the same way round) and joining the first point of α to the last point of the last vector.

and when it arises is seen instinctively to depend upon the "pull" in the cord which is drawing the bodies together. We seem to perceive, in fact, an alternative method of measuring the "force" in the cord; namely, by the rate of transference of momentum from one body to another which the cord in a given state of elongation mediates. The soundness of this method is capable of an indirect verification. Imagine a number of precisely similar cords to be suspended vertically and to be stretched, each to the same extent, by loads of different materials attached to their lower ends. Then we are bound to think of the elongations of the cords as being caused by equal "forces," the equal weights of the suspended loads. If at the same instant the loads are all set free and allowed to fall (*in vacuo*), we are bound once more to think that the circumstances of their motion are determined by the same "weight" which caused them to elongate the cords to the same degree in each case. If the forces in the stretched cords could be measured unambiguously not only by the weights which produce the given elongation but also by the rate of change of momentum of a body drawn along freely by a similar cord stretched to an equal degree; then it would be reasonable to expect that the different loads now supposed to be falling freely would exhibit equal changes of momentum. But since their weights are equal their masses are equal also. It remains to be shewn, therefore, that the falling masses gain velocity at the same rate irrespective of the material of which they are composed. As is well known, there is abundant experimental evidence that this is the case.

Making, finally, an easy abstraction from the complications introduced by the extended character (and consequent rotatory movements) of the bodies in our experiments, we are now prepared to hold with much confidence that when two "particles" are involved in a "dynamical transaction" the rates of change of momentum along the straight line joining them are at any moment equal and opposite, and that this rate

of transference of momentum is the complete expression of the "notice" which one particle is taking, at the moment, of the positional changes of the other.

§ 44.

We have thus reached by a route partly historical, partly hypothetical, the classical Newtonian position. The concept of a special type of transaction ideally supposed to take place between two mass-particles, is extended in the Newtonian mechanics to account for all happenings in the material world. But to effect this extension it is necessary to think of the behaviour of a given particle at a given time as an expression of the fact that it is "taking note" of the behaviour of every other particle in the universe. It is obvious that it cannot do so in the simple way which alone has been contemplated by us up to the present—the particle cannot exhibit at one and the same time a rate of change of momentum directed along each of the straight lines joining it to the other particles of the universe, and equal in each case to the rate of change of momentum exhibited simultaneously by the particle at the other end of the straight line. "The distinct and general formulation of the principle" by which (in modern terms) the actual rate of change of momentum exhibited by the particle is regarded as the "vector sum" of these elementary rates, is one of the "chief advances" which we owe to Newton.* But, as Mr. Russell has, in effect, pointed out in his penetrating criticism of the Newtonian doctrine,† the acceptance of the vector law involves an important widening of our concept of the way in which one material point may "take note" of the behaviour of others; for it is clearly only by a convention that we can regard this rate of change of momentum actually presented as the *sum* of rates of momentum

* Mach, *op. cit.*, p. 192.
† *Op. cit.*, Ch. LV.

none of which are present in the "resultant." The apparent further difficulty that it would be, in practice, impossible to effect the vector summation of the "notice" taken by a particle of all the other particles in the universe, disappears when we adopt a formula, like Newton's Law of Gravitation, which makes the rate of change of momentum exhibited by two particles at a given moment a function of the distance between them of such a character that for great distances it becomes negligible.

§ 45.

It is of more importance to note with Mr. Russell,* that in the present state of mathematical theory, it is no longer possible to think of a particle as *possessing* a "velocity" or an "acceleration," or (consequently) a rate of change of momentum. Returning to our single pair of particles, A and B, moving towards one another along a straight line, the Objective facts are that at the moments t_1, t_2, and t_3, A occupies the points P_1, P_2, and P_3; while B occupies the points p_1, p_2, and p_3. Now suppose that τ seconds after A leaves the point P_1, it occupies a point between P_1 and P_2 distant δ from P_1. Then the fraction δ/τ will have a definite numerical value which it is customary to call the average velocity of A during the time τ. It is clear that if we make this time shorter repeatedly we shall obtain a compact series of these fractions. Under these circumstances it will be possible, as a rule, to specify a number, V_1, which is the *limit* of this series of fractions; that is such a number that it is impossible that any number should lie between it and the compact series of fractions. This number is the velocity of the mass particle at the point P_1. Thus defined, it is obvious that the velocity is nothing that can be thought of as a *state* of A. If in a similar way the velocity of the particle B at the point p_1 be supposed to be determined, and to have the value v_1, then the doctrine of mass which

* *Op. cit.*, p. 473.

we have seen to be equivalent to Newton's doctrine asserts that the ratio of the numbers V_1 and v_1 (if the particles be supposed to start from rest at P_1 and p_1 as in the ideal experiment described on p. 100) characterises the behaviour of the two particles throughout the "transaction"; so that if when the particles are at any other pair of points, P and p, the velocities are V and v, the ratio of the changes in the values of the velocities, *i.e.*, the ratio $(V-V_1)/(v-v_1)$ will preserve the value which we have already defined as the inverse ratio of the masses of the particles. It follows from the well-known relations between the velocity of a point, the time it is in movement and the distance through which it passes, that the distances P_1P_2 and p_1p_2 will be again in this characteristic ratio; though their actual values will depend upon the "law" which gives the velocity of one of the points at different distances from the other. It will be clear, in fact, that in general a given pair of these second positions will imply a definite law of this kind; so that when we have determined two pairs of positions of the particles, such as P_1, P_2, and p_1, p_2, we have the *data* (namely, the mass ratio and the "law of distance" involved) which are alone necessary to determine the subsequent movements of the particles. Generalizing this result, we reach the conclusion that whenever a particle takes up at a definite moment a definite new point of space its occupation of this point may be regarded as a "taking note" of *any* two previous configurations of all the points of the universe, including itself, and, therefore, a taking note of all such configurations.

§ 46.

In view of the fact that we have ascribed only psychological value to the concepts of momentum and force, regarding them merely as instruments for the "economic description" of Objective facts, suspicion may arise that this view of the "causal law" which governs the behaviour of a mass-particle is conventional also, so that if another set of descriptive

notions were employed it might not necessarily be implied in the resulting system of mechanics. Hertz's mechanics* is a system in which the notion of geometrical connections between the mass-particles (supplemented when occasion demands by hypothetical "concealed masses") replaces the "forces" of the Newtonian secondary construction. For this reason Mach regards the Hertzian mechanics as an attempt to substitute *integral* laws for the differential laws that make up the ordinary science of mechanics.†

That this does not imply that the motion of a particle is determined by consideration of only one configuration of the system to which it belongs instead of by two, as in the Newtonian mechanics, follows from the consideration that Hertz's principles lead us to the assertion that under any given set of conditions supplied in the "integral" form of geometrical connections the particle will take the path of the least possible curvature. But to specify the "curvature" of a path we must make use of a second differential coefficient, just as we must to specify an acceleration. Thus, by an argument closely analogous to that of the preceding section it may be made clear that in this case also one configuration of the particles of the system must be thought of as determined by at least two previous configurations.

§ 47.

A very little consideration will show that this concept of the "notice" which one part of the universe takes of the rest must be applicable to other elements of the warp and woof of Nature besides the obvious movements of matter. We have seen, for example, that the science of Heat is built up upon a basis of correlation of the number series with the observed series of states of bodies which are called temperatures. We

---

* Hertz, *Principles of Mechanics*, Eng. trans. by D. E. Jones.
† Mach, *Science of Mechanics*, 2nd Eng. ed., p. 255.

have seen that under the guidance of the concept of "heat" as a substance which can be transferred from one body to another it has been found possible to unify the facts of temperature change by the observation that (in an ideal case where only two bodies, A and B, are concerned) changes in temperature of A bear a definite and constant ratio to the like changes of B. The analogy to the law connecting the changes of velocity of mass particles is obvious and might be taken as a support of the view that temperatures may be reduced to dynamical terms; the state which we call by the name temperature being really a "state of motion" of the particles of a body. But as, following Mr. Russell and the modern mathematicians, we have seen that velocity is not a state at all, it is clear that the analogy must be differently conceived. The temperature of the body A at a given moment is the "notice" it takes of two preceding pairs of simultaneous temperatures of B and of itself, just as the position of a particle is the "notice" it takes of two preceding configurations of the universe to which it belongs.*

The only conclusion that it seems legitimate to draw from the consideration of this and other similar cases is that the law we have been studying is the necessary condition that must be present if it is to be possible to regard one kind of changes in bodies as expressions of the "notice" taken by them of changes of a different order elsewhere. It seems safe to assert that no order of phenomena which does not exhibit this law can be brought into relation with one in which the law obtains, but it would, perhaps, be dangerous to maintain that such orders do not exist.

* I have ignored in this discussion the fact that the "specific heat" of bodies is only approximately constant. The recognition of this fact would make the argument more complicated, but would not affect it essentially. *Cf.* J. J. Thomson, *Applications of Dynamics to Physics and Chemistry*, 1888, p. 90.

§ 48.

Whether, as Mach would have us believe,* Newton was well aware that his concepts of *quantitas motus, vis impressa,* and the like are merely of psychological value in connection with the investigation and transformed statement of actual facts, or whether he attributed to them objective validity, is a matter upon which it would be neither easy nor profitable for us to come to a decision. There is no doubt whatever that the latter is true of the majority of his contemporaries and successors, at the very least until the time of D'Alembert, if not, indeed, until the present day. Thus allusion has already been made to the famous controversy between the followers of Descartes and Leibniz, upon the subject of the "correct" method of measuring that "force" of a body in motion which we have noted as one of the vague notions in which the science of mechanics has its birth. From the standpoint of the development of the science, the great value of the controversy was that, by concentrating attention upon the theoretical aspect of the method by which individual problems in mechanics were solved, it led to an elaboration of definite bodies of mechanical doctrine with a definite nomenclature. From the critical point of view, the controversy is most interesting as throwing light upon the way in which the most pretentious and far-reaching scientific doctrines are based upon a mass of vague experiences, of "instinctive perceptions," the precise nature of which it is not always given to the founders of the science to recognise for what it really is. Thus even Thomson and Tait† admit that Newton's "axioms" "must be considered as resting on convictions drawn from observation and experiment, *not* on intuitive perception." But Newton, like Gauss, exhibited his edifice of thought to the world only "when the scaffolding had been removed," and has left us no precise indication of the

* *Science of Mechanics*, p. 193.

† Thomson and Tait, *A Treatise on Natural Philosophy*, 1867, p. 178.

"observations and experiments" from which he drew the conviction that the product of mass and velocity was the determining circumstance in dynamical transactions. Hence we have found it advisable to devise a hypothetical series of experiences by the consideration of which this conviction might be reached. It is otherwise with the conviction that the "*vis viva*," the product of the mass into the square of the velocity, is the determining circumstance.

§ 49.

In 1669 Huygens acquainted the world, by means of the Philosophical Transactions of our own Royal Society, with his laws of motion of colliding elastic bodies. In 1703 a posthumous *Tractatus de motu corporum ex percussione* was published,* in which these laws appeared systematically deduced upon the basis of certain assumptions, which as a rule are of the kind called by Mach "instinctive perceptions," and are clearly set forth.

Of this character, for example, is the assumption that the velocities, relative to an observer, with which two colliding bodies separate, will not be changed if the two bodies *and* the observer have an additional common velocity imposed upon them—as they would have, for instance, if the experiment were performed upon a uniformly moving boat.† But when Huygens reaches the case of elastic bodies of unequal mass, and endeavours to prove the proposition‡ that if such bodies collide with velocities inversely proportional to their masses, they will separate with the same velocities, he finds himself able to do so only by showing that a contrary supposition is forbidden by an axiom of complete certainty in mechanics, "which states that in a movement of bodies which is caused by their own weights,

---

* Translated as No. 138 of Ostwald's *Klassiker der exakten Wissenschaften.* I have used this translation.

† *Tractátus*, p. 370.

‡ No. VIII. Pp. 381–6.

their common centre of gravity cannot possibly rise."* Here we have an appeal to a principle, used again with great effect in Huygens' famous solution of the problem of the "compound pendulum,"† which, in the hands of the Bernouillis, developed into the Principle of *Vis Viva*,‡ which Helmholtz made his starting point in his epoch-making work on the "Conservation of Energy."§ The same instinctive perception that a body or a system cannot, under the influence of its own weight, move in such a way that the centre of gravity is raised—a conviction which is recognised as identical with the denial of the possibility of the *perpetuum mobile*‖—was shown by Mach¶ to underlie the Principle of Virtual Work (or Velocities) which Lagrange made the basis of his *Mécanique Analytique*, and hence of the method of "generalized co-ordinates" which plays so important a part in modern mathematical physics.**

§ 50.

Helmholtz starts with the Newtonian concept that the task of physical science is to reduce the phenomena of Nature to unchanging, attracting, and repelling forces acting between mass-points; the magnitude of the forces depending merely upon the distances between the points. He shows†† that in such a system of mass-points and forces the *vis viva* will be

---

* *Tractatus*, p. 382.

† *Horologium oscillatorium*, 1673. The solution is described by Mach, *op. cit.*, pp. 174–9.

‡ Huygens had, in the *Regulæ* of 1669, already formulated for collisions of perfectly elastic bodies the rule: "Summa productorum a mole cujuslibet corpori duri, ducta in quadratum suae celeritatis eadem semper est ante et post occursum eorum" (Reg. 6, *Phil. Trans.*, No. 46, p. 928).

§ Helmholtz, *Ueber die Erhaltung der Kraft*, 1847.

‖ *Cf.* Helmholtz, *op. cit.*, p. 8; also Mach, *Popular Scientific Lectures*, Eng. trans., 1894, p. 147.

¶ "On the Conservation of Energy" in *Popular Lectures*, 1894.

** *See* Routh, *Rigid Dynamics*, i, Ch. VIII.

†† *Op. cit.*, pp. 10–12.

conserved: meaning by this statement that whenever a particle returns to a position which it has temporarily left in an otherwise constant configuration, its *vis viva* will regain its old value. This somewhat narrow principle is extended in the next section* to the case when the particle, still under the action of central forces, has not yet returned to its original position. In this case, by distinguishing a new quantity (to which Rankin gave the name "potential energy" † while calling Helmholtz's *vis viva* "kinetic energy") he was able to show that the *sum* (as we should call it) of the kinetic and potential energies was constant. In this way Helmholtz reached the Principle of the Conservation of "Force" [Energy] for the case of particles under the action of central forces. The next two sections of the work are devoted to the consideration first of the cases in which the principle of conservation is already implicitly known to apply, and next to the most interesting cases‡ (such as inelastic collision and friction) which have hitherto been thought to be cases of an absolute loss of "force" [energy]. It is in this chapter that Helmholtz brings into line with his own systematic mathematical treatment the experiments in which Joule has shortly before obtained evidence of an "equivalence" between work and heat; and so, for the first time, makes it appear possible that the hitherto autonomous province of the science of Heat will be annexed by the expansion of the classical Newtonian Mechanics.

§ 51.

But the doctrine of the Conservation of Energy, although it formulates conveniently a connection between dynamical events and temperature changes, does not of itself effect the reduction we are seeking. It does not, that is, enable us to describe

---

* *Op. cit.*, pp. 13–19.

† *See* Merz, *op. cit.*, ii, p. 139.

‡ *See* Merz, *op. cit.*, pp. 25–6.

transactions in which temperature changes occur in terms of motions and mass-coefficients only. The critical treatment to which we have subjected the concepts, kinetic energy and heat, has been sufficient to show that they belong to the order not of primary facts but of secondary constructions. The substantial shape we give them is legitimate if we remember that it makes the results of experimental or mathematical investigation "psychologically available." But the identification of heat as a "form" of energy has no *real* value; merely extending, in fact, the psychological usefulness of the substantial conception of energy to render intelligible the *fact* that a change in the value of the arithmetical quantity $\frac{1}{2}\Sigma mv^2$ may at times be accompanied by the appearance of temperature changes under conditions which are conventionally described by the statement that a quantity of heat has appeared or disappeared which bears a definite ratio to the change in the value of the former quantity.

§ 52.

The correlation of temperature states with definite quantities of mechanical work theoretically effected by Lord Kelvin's absolute thermodynamic scale, would appear at first sight to be, in effect, equivalent to the desired reduction. Lord Kelvin's argument* showed that in a *reversible* heat engine, the "efficiency" (that is, the ratio of the quantity of heat converted into work by the engine to the total quantity which it draws from the "source") depends upon nothing but the actual temperatures of the source and the "condenser." Suppose, then, two reversible engines drawing heat from the same source A but returning it (less the equivalents of the work done by each) to different condensers B and C. Let the engines work until each has drawn the same total quantity of heat from A. Then the amounts of work done will depend merely upon the temperatures of B

* *See* Preston, *Theory of Heat*, p. 611 *et seq.*

and C, and may be taken therefore as measures of the differences between these temperatures and that of A. Thus, if the second engine does twice as much work as the former, the difference of temperature between A and C may conveniently be taken to be twice as great as in the case of A and B. We may assign, then, any arbitrary number to the temperature of A, and in this way correlate with any standard temperatures available (such as the boiling and melting points of pure substances), numbers which will be independent of the properties of any particular substance. It will be convenient, however, so to choose the number assigned to the temperature of A that in the ideal case in which *all* the heat drawn from the source is converted into work, the number assigned to the temperature of the condenser shall be zero.*

§ 53.

Unfortunately, however, this correlation of temperatures with quantities of mechanical work does not enable us to dispense with the notion of temperature as a definite state of a body which is the object of our sensations of hotness or coldness. The argument which made this unambiguous correlation possible is not developed from mechanical *data* only, even if we ignore the difference between heat and kinetic energy. It involves our direct experience (formulated in the axioms of Clausius and Lord Kelvin) that hot bodies in the presence of colder ones get cooler, never hotter, except (ultimately) by the intervention of animate agencies.†

To effect the complete annexation and assimilation of the science of Heat, Dynamics must, then, give some account of the behaviour of solid and liquid bodies from which (as in the Kinetic Theory of Gases) this irreversible property of temperature can be deduced.

---

* The "absolute" zero.

† Preston, *op. cit.*, p. 612.

The most important attempts in this direction have been reviewed by Professor G. H. Bryan in an important report "On our Knowledge of Thermodynamics, with particular reference to the Second Law."*

There are three conditions which must be satisfied by any dynamical analogue of temperature. It must play the part which temperature plays of determining the changes in the system (when it is brought into contact with another system) which result in "thermal equilibrium"; it must be shown that, with the dynamical analogue, systems which are "at the same temperature" as a third system, are in "thermal equilibrium" with one another; lastly, the analogue must exhibit the property known sometimes as Clausius' Theorem, sometimes as the Second Law of Thermodynamics. This law states that, for all *reversible* transformations which a body undergoes, if the energy supplied in the form of heat be divided by the absolute temperature at which it enters the body the quotient will be a "perfect differential." This expression implies that whenever the body undergoes a series of changes of pressure, volume, and temperature, the integral† of the quotient in question throughout the series defines an unambiguous change in the body called a change of *entropy*, which is independent (if the condition of reversibility is maintained) of the route by which the body passes from its initial to its final state. If we concern ourselves only with the quantity of heat given and make no reference to the absolute temperature at which it passes into the body, we do not possess the *data* for determining unambiguously the final state of the body. Hence the absolute temperature (which converts the

* *B.A. Report*, 1891, pp. 85 *et seq.*

† Suppose the series of changes to be broken up into any number of sections. Also suppose the heat received during each section to be divided by the absolute temperature at the beginning of that section, and the sum of the quotients to be taken. As the number of sections is increased the sum will approach a *limit* (p. 103). This limit is the integral.

increment of heat into a perfect differential) may be called an integrating divisor. The third property of (absolute) temperature may be expressed, then, by saying that it must be an integrating divisor of the energy which is given to the system as heat.

§ 54.

Almost the only writer who has attempted to devise kinetic analogues which shall satisfy at once all these conditions is Helmholtz,* whose "monocyclic systems" can be shown to do so if certain assumptions are allowed.

In view of the account which we have already given of the part which Helmholtz played in the derivation of the principle of the conservation of energy as an extension of principles founded on the postulate that dynamical transactions can be reduced to attracting and repelling forces between mass-particles,† it is interesting to find that he has attempted a dynamical interpretation of temperature by a method that avoids the postulation of the existence of an infinitely large number of molecules. This can be done by means of assumptions which express the kinetic and potential energies of the system in terms of the amounts and variations of a number of *co-ordinates*—a term which, by a usage introduced by Lagrange, may be extended from its simple geometrical significance to any quantity which can be used to fix the spatial configuration of a system.‡

But the whole difficulty of the present case is the fact that we are ignorant of the "co-ordinate" which describes the configuration of the molecules when a body is at a definite temperature. The molecules are, in fact, "concealed masses," "whose position still remains unknown when the co-ordinates accessible to observation have been completely specified."§

---

* Bryan, *loc. cit.*, p. 104.
† *Supra*, § 50.
‡ Bryan, *loc. cit.*, p. 96.
§ Hertz, *Mechanics*, p. 223.

Their configuration, therefore, is to be fixed by what Professor J. J. Thomson has called "unconstrainable" co-ordinates, in distinction from the "controllable" co-ordinates which are subject to the direct control of an experimenter.* Finally, by supposing the "concealed masses" to form a "cyclical system," that is, to possess constantly recurrent motions, it is possible to "ignore" the unconstrainable co-ordinates, and deal only with their time-rates of change.†

(A simple example of a monocyclic system in which the concealed motion is determined by one "unconstrainable" co-ordinate, would be a perfectly uniform rotating wheel. The angular position of a point on the wheel would be the "unconstrainable" co-ordinate sufficient to fix the position of the wheel at a given moment, while its time-rate—the angular velocity of the wheel—would alone affect the expression for the kinetic energy of the system.)

The main features of the course of Helmholtz's somewhat recondite argument are as follows:—An expression is first obtained for the additional energy that may be imparted to a simple monocyclic system‡ by means of the concealed masses only. This energy corresponds to the heat given to raise the temperature of a body. It is next shown that the *whole* kinetic energy of the concealed masses is an integrating divisor of the expression for this additional portion of energy. Thus it appears that the kinetic energy of the concealed masses—or a product of this kinetic energy by any one of a certain class of functions§—possesses the property which Clausius' theorem indicates as distinctive of absolute temperature.‖ Helmholtz

---

* Thomson, *Applications of Dynamics to Physics and Chemistry*, 1888, p. 94.

† Thomson, *Applications*, p. 14; Hertz, *Mechanics*, p. 209.

‡ *I.e.*, a system containing only one set of cyclical movements, which can themselves be defined by one co-ordinate.

§ Functions of the "generalized momentum" of the system.

‖ *Supra*, p. 113.

next succeeds in showing* that one of the products just mentioned can always be found which is an integrating divisor of the energy of the compound cyclical system built up by coupling any two given monocyclic systems together. This result is easily interpreted as the analogue of the common temperature at which two bodies are in thermal equilibrium.† Finally, the investigation proves that these systems possess the third property of temperature—that if two of them can each be coupled with a third, they can be equally well coupled with one another.‡

In this way Helmholtz has shown that all the thermodynamical properties of matter can be represented dynamically by means of monocyclic systems which are capable of being coupled together. But owing to the somewhat arbitrary nature of the assumptions which the argument makes, it cannot be said that he has *proved* a dynamical origin for temperature§ —even in the modified sense that he has established other Objective facts of a dynamical order which constantly accompany it. Moreover, the investigation has only attempted to cover *reversible* heat phenomena, and appears to be incapable of including the cases of irreversible phenomena, to which actual experience practically confines us.‖ We may conclude, then, that even the most complete and successful attempt that has been made to reduce temperature to a dynamical phenomenon, not only reaches its end by inspired assumptions instead of by the road of inevitable deductions, but also fails to consider the enormous bulk of the primary facts in question. It does not, therefore, escape the condemnation which Professor Bryan

* Bryan, *op. cit.*, p. 102.

† As a simple instance of such dynamical analogues, Helmholtz gives the case of two revolving wheels which may be coupled together by joining their axles if their angular velocities are equal. (Bryan, *op. cit.*, p. 101.)

‡ Bryan, *op. cit.*, p. 103.

§ *Op. cit.*, p. 104.

‖ *Op. cit.*, p. 108.

pours on all the attempts which he passes under review, that they seek "to prove the Second Law, about which we know something, by means of molecules, about which we know much less." *

§ 55.

It would appear that even the partial success which has been reached in the attempt to unify by means of dynamical concepts alone the phenomena which lie in the old provinces of Heat and Mechanics, is dependent, at least in the present state of science, upon the use of methods which (in Professor J. J. Thomson's words)† "do not require an intimate knowledge of the system to which they are applied." If we are modest enough to abandon the hope that we may, by the application of dynamics, "discover the properties of such systems [as are investigated in physics] in an altogether *à priori* fashion," we may yet most usefully employ dynamical methods "to predict their behaviour under certain circumstances after having observed it under others."‡ "The way in which dynamical considerations may enable us to connect phenomena in different branches in physics," is illustrated by Professor Thomson in the following manner:

"Let us suppose that we have a number of pointers on a dial, and that behind the dial the various pointers are connected by a quantity of mechanism of the nature of which we are entirely ignorant. Then if we move one of the pointers, A, say, it may happen that we set another, B, in motion.

"If, now, we observe how the velocity and position of B depend on the velocity and position of A, we can by the aid of dynamics foretell the motion of A when the velocity and position of B are assigned, and we can do this even though we are ignorant of the nature of the mechanism connecting the

* *Op. cit.*, p. 121.
† J. J. Thomson, *Applications*, p. 8.
‡ Thomson, *op. cit.*, p. 5.

two pointers." As the generalization of this example we read that "the observation of the motion of B when that of A is assigned may be taken to represent the experimental investigation of some phenomenon in Physics; while the deduction by dynamics of the motion of A when that of B is assigned may represent the prediction . . . of a new phenomenon which is a consequence of the one investigated experimentally."*

For the application of the method contemplated—which is a development of Hamilton's or Lagrange's principle—it is only necessary to suppose that the system is possessed of a constant amount of energy which expresses itself by various "co-ordinates," a term, originally of a strictly limited geometrical significance, which was extended to any quantities which can be used to determine the position of a body in space† or the spatial configuration of the parts of a system‡ and in the present connection is further extended to elements which "fix the configuration" of the system in a wider sense, such as elastic strains, electrical and magnetic phenomena. This constant quantity of energy may be regarded either as the sum of the "kinetic" and "potential" energies of the system, or (in view of the fact that potential energy is a somewhat unsatisfactory term, corresponding directly to nothing Objective)§ as the sum of the kinetic energies of the given system (determined by the "positional co-ordinates") and of subsidiary connected systems such as the ether (determined by "kinosthenic" or "ignored" co-ordinates). It is clear that terms in this expression for the energy of the system may be contributed by each of the co-ordinates that fix its configuration, and that the coefficients of such terms may involve any of the other co-ordinates, and possibly more than one at the

* Thomson, *op. cit.*, p. 6. *Cf.* Rayleigh, *Theory of Sound*, pp. 71 and 75.

† Routh, *Rigid Dynamics*, i, p. 57.

‡ Rayleigh, *op. cit.*, p. 67.

§ *Cf.* Bradley, *Appearance and Reality*, pp. 384 *et seq.*

same time. For example, the part of the kinetic energy of a small sphere which is due to its velocity is affected, not merely by its mass but also by the electric charge which it may bear, the effect being the same as if its mass were increased. At the same time the presence of the charge adds an independent term to the expression of the energy. Applying to this expression the operations prescribed by Lagrange's equation—regarding the electric charge as the variable—it is easy to deduce the value of a quantity which must bear the same relation to the energy due to the electric charge as ordinary force does to ordinary kinetic energy.

By an extension of its original meaning, based upon this analogy, and parallel to the extension of the meaning of the term "co-ordinate," we may call any such quantity a "force" of a special "type," determined by the particular co-ordinate with regard to which* the differentiations have been performed. In this case the force will be an "electro-motive force," which will involve the velocity of the sphere. But this result implies that the "electrical capacity" of a moving sphere is different from that of the same sphere at rest, a result which before was unknown.† Thus from the known presence of a certain term in the formula for the energy, a term which expressed a known connection between two phenomena—the amount of the electrical charge on a moving body and its apparent mass—we have been able to deduce a reciprocal effect previously unknown.

It is important to notice that in Professor Thomson's equations the energy is always expressed as a quadratic function of the co-ordinates or their rates of change. It follows that the "generalized forces" which are obtained by Lagrange's method contain either second differential coefficients analogous to acceleration, or else quantities analogous to

---

* And its differential coefficient with respect to the time.

† Thomson, *op. cit.*, pp. 31–4.

differences of temperature. In accordance, then, with the results we reached in §§ 45—7, both these types of co-ordinates must be subject to the law that the value assumed by one of them at a given moment can be regarded as an expression of the "notice" taken by the system or a part of the system of two previous configurations.

§ 56.

The consideration of this example may suffice to illustrate the fact that the sphere of operations of the method can only include a given co-ordinate when it is possible, in the assumed absence of other co-ordinates, to express the *whole* of the energy of the system in terms of the former. Consequently when Professor Thomson approaches questions in which temperature effects are involved* it is necessary for him to form and to justify some definite concept of the relation of the whole energy of a body to its temperature, when the body is *merely* hot, and is not the seat of an electric charge, of elastic strain, etc.† Thomson follows Helmholtz in assuming that the quantity analogous to temperature is the kinetic energy due to the molecular or unconstrainable co-ordinates. It is unnecessary, therefore, to do more than quote Professor Bryan's criticism, that in Professor Thomson's argument "properties of temperature are assumed which . . . have *not* hitherto been satisfactorily deduced from dynamical principles."‡

§ 57.

In face of mathematical difficulties that have baffled even such giants of analysis as Helmholtz, Maxwell, and Thomson,

---

* *Op. cit.*, Ch. VI

† So long as the specific heat of a body is constant the rise of temperature may be (at constant volume) a measure of the energy given to it in the form of heat. This fact alone does not enable us to express as a function of the temperature the whole energy of the body.

‡ Bryan, *loc. cit.*, p. 121.

it is not surprising that investigators have turned aside from "the high *priori* road," and, accepting the Carnot-Clausius theorem as a law of our experience which holds good of all transactions in which temperature changes occur, have sought to unify the phenomena of the provinces of mechanics, chemistry, and physics into one science of "energetics" which, contenting itself with measuring the bare faces of sensible phenomena,* expressly declines to attempt to "explain" one set of objective phenomena in terms of another—and so comes to exhibit a practice that accords with the philosophical tenets of this essay.

The essential features of this method may be summed up by the statement that it seeks some principle of determining equilibrium in the "configuration" of a system (taking this term in the wide sense explained on p. 118) that shall be equivalent to the principle of virtual velocities which is fundamental in dynamics, yet shall not, like the latter, be expressed in an idiom peculiar to one of the special sciences whose particulars are to be related.

The argument which seeks this goal starts from the experiences summarized in Clausius' law that if a system goes through a series of modifications of a *reversible* character its entropy suffers an unique and unambiguous change which depends only on the initial and final states of the system;† while if the modifications are *not* reversible the change of entropy which would have occurred under the conditions of reversibility will be exceeded by an amount which Clausius called the "sum of the non-compensated transformations."‡ If now we agree to restrict our attention to cases in which the transformations occur at a constant temperature, this additional quantity of entropy can be regarded as corresponding to

---

* James, "Humanism and Truth," *Mind*, N.S., No. 52, p. 459.

† *See* p. 113, *supra*.

‡ Duhem, *Le Potentiel Thermodynamique*, 1886, p. 6, from which, in the main, this section is derived.

a definite amount of "non-compensated work." It is not difficult to obtain a mathematical expression for this work involving the change of entropy (under reversible conditions) the "internal energy," the (constant) temperature of the body in question, and the external work done during the transformation. Finally, after assuming limitations to the character of the external work conformable with the conditions under which an investigation of this kind would commonly be pursued, it becomes possible to regard the "non-compensated work" as the difference between the values at the end and the beginning of the operation of a special function which its inventor, Duhem, calls "the thermodynamic potential."* In this function we have, for a large number of important cases in which "physical phenomena affect one another," the desired means of specifying the state of equilibrium. For "lorsque le potentiel thermodynamique est minimum, le système est dans un état d'équilibre stable."†

It would be unsuitable to follow in these pages the extremely technical development and applications of this principle. It must suffice to note that by means which are equivalent to the use of the thermodynamic potential, but before Duhem had published his systematic theory of the processes, Willard Gibbs had shown how to reduce to intelligibility large areas of previously intractable chemical and physical phenomena; while Helmholtz had explained, by thermodynamical reasoning, the astonishing discovery that the heat developable in an electric circuit is only a fraction of the heat equivalent of the chemical decompositions occurring in the cell—a fact which he showed to be a particular case of the law that only part of the heat drawn from any source is available for transformation into useful work. This part, to which he gave the name of "free energy," is identical (in this case) with Duhem's thermodynamic potential. Upon the work of these pioneers—

* *Op. cit.*, p. 8.
† *Op. cit.*, p. 9.

particularly upon that of Willard Gibbs—so great a theoretical fabric has been erected in recent years that chemistry is rapidly becoming absorbed into the circle of the mathematical sciences.

§ 58.

The results of the last two chapters may now conveniently be summarised. Starting from the concept of Science as a conative process, which aims at rendering Objective facts intelligible to an individual consciousness, and reaches that end by building up those primary facts into secondary constructions or apperceptive systems, by means of ideas drawn from other contexts of experience, we took up the positions (1) that no concept is to be deemed essentially incapable of rendering primary facts intelligible on the ground of the context of experience from which it is drawn; and (2) that in no case is the concept or hypothesis to replace, in the sense of accounting for the "reality" of, the Objective facts which it has been employed to render intelligible.

With regard to the first position, we came, in effect, to the conclusion that each science must be (to use an expression employed somewhat similarly by Professor Bosanquet) "self-normative" with respect to the hypotheses it uses; certain hypotheses being ancillary to progress at one stage, or in one sub-province of the science, others at another stage, or in another sub-province. The only general statements of a normative character that we could make are that the secondary construction must be a genuine reaction upon the primary facts, and must not be forced upon those facts *ab extra;* and that, while such concepts as "end" are, or may be, appropriate at certain stages of the science, progress will show itself in the adoption of hypotheses which suggest and make possible "a complete synoptic view" of the primary facts, that is of hypotheses which can subserve a mathematical treatment.

With regard to the second position, we examined in the present chapter, of course very briefly, though, it may be hoped

not inadequately for our purpose, the most systematic of the attempts that have been made to render the whole range of sensible facts intelligible by means of the concepts of "mass" and "motion" which are themselves drawn only from one province of primary facts. While, even if this attempt had been successful, we should not have felt compelled to admit that the Objectivity of the sensible facts of hotness and coldness had been in any way affected by the discovery of other Objective facts that were found to have a constant relation to them, yet, as a matter of fact, we drew the conclusion that the reduction of the secondary qualities to primary qualities, which is one of the chief motives of this attempt, has not been effected, and that the failure to effect it has encouraged the development of methods of dealing with Objective facts which respects their Objectivity. Using one of Lotze's pregnant phrases, we have regarded as the goal of this method the employment of correlation of the facts with the members of the number series as a means of analysing and recording the performances of things, so as to detect the manner in which one Real "takes note" of the behaviour of others with which it has relations. The establishing of these relations, when the primary facts which are the expression of them lie in different provinces, has been most powerfully aided by the concept of Energy, which attributes, in effect, the "*quelque chose qui demeure constant*" which is the form which our conviction of relationship inevitably takes,* to the passage of a substance (in the strict philosophical, together with an admixture of the ordinary, sense) from one body to another, where it may appear in a transformed shape. In accordance with our general doctrine, while admitting the psychological usefulness of the concept, and, indeed, insisting upon its psychological necessity, we must refuse to allow writers like Ostwald, against whom we have actually been defending hypotheses, to regard it as the one reality to which

* Poincaré, *Science et Hypothèse*, 1902, p. 153.

we can reduce not only "things" and their "qualities," both primary and secondary, but even, perhaps, psychical existences as well.* Having served its purpose of leading to the most complete "synoptic inventory" that physical science has reached, it must, unless its Objectivity (in the sense in which we use that term) has been incidentally established, resign the claim to belong to the realm of facts which it has served both to render intelligible and to increase, and be contented to be recognised as merely the powerful assimilative and expository instrument that no one will deny it to be.

* Ostwald, *Naturphilosophie*, esp. Ch. 18.

CHAPTER V.

§ 59.

The pre-critical view that in certain concepts of Science we reach the realities which lie at the back of perceived phenomena, is one which will always have an attraction for the actual workers in Science. It implies, perhaps, a certain aloofness from practical life to resist conclusions supported by evidence upon which one would act with confidence even in affairs of the highest moment. From this point of view Huxley* pours ridicule upon those who would decline to accept the geologist's reading of the palæontological record. If they were consistent, he argues, they would decline to draw the usual conclusions from the oyster shells outside the fishmonger's door, or the mutton bone in the dust-bin.

In the class of cases which Huxley adduces there are few who would reject his conclusions; there are few of us, again, who would be satisfied, as Professor Karl Pearson leads us to suppose that he would be,† to "describe and classify [our] immediate sense-impressions and [our] stored sense-impresses by the aid of the theory of evolution," even "had the universe been created just as it is yesterday"; or with a theory of matter upon which the negative "ether-sinks" (to which nothing perceptual appears to correspond) "would long ago have passed out of the range of ether-squirts" (which correspond to perceptual matter), so that we need not concern ourselves about their fate. There are few, I repeat, who would not be troubled with "obstinate questionings" as to the *truth* as well as the "economy" of these conceptions. The scruples

* In his lecture "On the Method of Zadig," *Science and Culture*, p. 139.

† Pearson, *The Grammar of Science*, 1st ed., pp. 418 and 319.

of such seem to imply the conviction—conscious or unconscious—that the business of Science is, as I have so often insisted, to render the Objective intelligible, and that the Objective thus systematised must ultimately be the whole Objective and nothing but the Objective.* No gap in either the spatial or the time series is to be tolerated, nor can we suffer any place in either of the series to be filled by the hypothetical masquerading as Objective.

But this principle, apparently so simple and so clear, discloses unsuspected difficulties of application when we try to determine by its aid the precise value and import of the concepts by means of which we seek to make accessible Objective phenomena intelligible. Many of these concepts assign positions in the spatial and temporal series to things which it is either essentially or else practically impossible to verify. "Attraction" is an example of the first class, "atoms" of the second. What is the actual standing of such entities? It cannot be denied that *some* of the evidence is forthcoming which, if completed, would establish their existence, and if this evidence actually produces conviction in men of the highest intellect supremely conversant with the facts, what more is to be said? The denudation which "the Razor of Occam" would produce would depend entirely upon the hand that wielded it. If it were applied by Lord Kelvin, the ether, for example, would be safe; if by Professor Karl Pearson, its fate would be at least doubtful.† If it were handled by Professor J. J. Thomson, the "Faraday tubes" would disappear, while "ions" would, I imagine, remain. The truth seems to be that while cases of this kind were few and isolated, men's attitude towards them might be indeterminate—each case was judged upon its merits. But when with the advance of Science a whole compact system of concepts appeared claiming to represent what "goes

---

* *See* Sigwart, *Logic*, ii, § 61.

† *Grammar of Science*, 1st ed., p. 214.

on behind what we see and feel" over the whole surface of the Objective, it became inevitable that individuals should take up a definite general attitude towards them, only to be abandoned exceptionally: that is, that they should adopt a more or less explicit philosophy of Science. For those who accepted the claims of the new concepts, "atoms," "energy," "ether," and the like became metaphysical terms, the names of ultimate realities, or of an hierarchy of realities, of which what we have described as the Objective is only the appearance. As metaphysical entities it was inevitable that they should eventually claim to be able to account for the whole of experience. Thus was developed that "mechanical philosophy" which has recently suffered such a severe cross-examination by the author of *Naturalism and Agnosticism.*

§ 60.

We may, it would seem, distinguish usefully between three distinct types of "secondary construction" to all of which the name hypothesis has been indifferently given. In the first kind the *data* are a number of facts of experience which form an incomplete spatio-temporal system of a familiar type. The hypothesis here simply suggests additional elements of the same order that would make the system complete. A detective's hypothesis of a crime is of this kind. It interpolates between the *data* or "clues" other spatio-temporal facts of the same order as the clues, which with the latter would make a system which from its conformity with our experience would be felt to be complete. The hypothesis that the collection of fossil bones labelled *Diplodocus Carnegii* in the Natural History Museum once formed the skeleton of an enormous living reptile, is another example of the same kind of hypothesis. It is evident that the spatio-temporal links which such an hypothesis introduces between the *data* are always of such a character that they might at least conceivably have been verified. Thus some museum on the surface of Mars might conceivably contain, besides a

duplicate skeleton, a sufficiently authenticated photograph of Diplodocus in the flesh, a *souvenir* of a Martian visit to Earth in Jurassic times! At any rate, there can be no doubt that the man who feels that the evolutionary hypothesis gives a satisfactory explanation of existing biological facts, believes in the vast majority of cases that it does so because it supplies the "missing links" of a spatio-temporal chain, all of which could have been verified by a human observer if he had been present.

§ 61.

An important sub-class of this type is formed by hypotheses of an "ejective" character. The counsel in the court of law who seeks to persuade the jury that the accused was actuated by certain motives, or had a certain intention, is employing an hypothesis of such a kind. It is clear that it has the marks of conformity with our experience and (in a certain sense) of homogeneity with the facts between which it is interpolated. On the other hand, it is essentially unverifiable. Whether such hypotheses as "attraction" or the concept of "vital force" should be included here is doubtful. They can hardly be so included unless they can be said in the given cases to be interpolations conformable with our experience. Thus *we* are undoubtedly conscious of attraction and repulsion, but are we, therefore, entitled to "eject" them into the matter of a planet? Much depends here upon the general character of our convictions. Thus Gilbert of Colchester could write in 1600, "Miserable were the condition of the stars, abject the lot of the earth, if that wonderful dignity of life be denied them, which is conceded to worms, ants, moths, plants, and toadstools."* From such a standpoint there can be little doubt that "attraction" falls fairly into the present class of hypotheses. For the modern physicist who belongs to a generation that has learnt to disbelieve in consciousness where there is no evidence

* *De Magnete* (Eng. ed., publ. by the Gilbert Club, 1900, p. 209).

of nervous tissue, the notion belongs just as clearly to the third class to be considered below.

§ 62.

In the second type of hypothesis the elements which are added to make the secondary constructions are not spatio-temporal existences but relations between such existences. Such an hypothesis was Newton's belief that the attraction of the earth for the moon could be calculated from its attraction for a stone on the earth's surface in accordance with the law of inverse squares, or Joule's conviction, maintained for years in spite of contradictory experimental results, that a definite equivalence existed between heat and work. Hypotheses of this kind share with the former type the characteristic of being, at least ideally, verifiable.

§ 63.

In the last class we find the typical hypothesis of science as opposed to the hypothesis of history and common sense, the hypothesis which Ostwald has attempted to banish from scientific method. In general, its marks are—(1) a lack of the homogeneity between the *data* and the added or interpolated elements which characterised the first type; (2) the *unverifiable* character of the added elements; and (3) that the secondary construction does not merely *complete* the *data* but actually replaces them.

The first two of these marks, at least, are present in the case of the hypothesis of *heat*, by means of which temperature changes are explained. The entity which is thought of as "flowing" from the hot body to the cooler body is not thought of as of the same order of existence as the sensations of hotness and coldness which are the actual *data* here; and quite obviously it is completely unverifiable—no one has ever pretended to exhibit heat apart from the phenomena of hotness and coldness which it is invoked to render intelligible.

The concept of molecule as used to explain physical and chemical phenomena appears to be in possession of all these marks. The *data* are the modes of behaviour of molar bodies which cannot be regarded as homogeneous with the modes of behaviour of the individual molecules of which the former are assumed to exhibit only the statistical result.* Secondly, if physicists' calculations of the "size" of molecules and their conclusions as to the "wave-length of light" are both to be accepted, molecules *must* in all probability be unverifiable.† Finally, in this case it is one of the expressed "objects of Physical Science to explain natural phenomena by means of the properties of matter in motion,"‡ where by "explain" is probably meant to exhibit the reality of which the phenomena in question are only the appearances.

§ 64.

On the question of the validity of these various types of hypotheses our own doctrine is clear. The first and second classes consist of hypotheses which suggest definite interpolations where interpolations are demanded by our previous experiences. The "other context" from which an hypothesis of this type is drawn consists of experiences of the same class as those which the hypothesis ideally completes. This completion, as such, is the sole object of such hypotheses, which, when verified, become merged in the Objective facts which they have served to make intelligible. None of these things can be said of hypotheses of the third class. They are interpolated where there is little or nothing to warrant interpolation.§ They are drawn from

* *Cf.* in particular Ward, *Naturalism and Agnosticism*, i, pp. 92–111.

† I leave it to the competent to decide whether this does not make the assumption of their existence self-contradictory.

‡ J. J. Thomson, *Applications of Dynamics to Chemistry and Physics*, p. 15.

§ Personal opinions will always differ as to the amount of such warrant in particular cases—*e.g.*, in interposition of an "ether" to explain action at a distance. *Cf.* Larmor, *B.A. Report*, 1900, p. 627.

contexts of experience which are not of the same class as the phenomena in question. Finally, so far from becoming merged in the Objective facts which they render intelligible, we have seen that their fate is to disappear altogether when they have enabled us to arrive at a "complete synoptic inventory" of these facts.

In support of this view many arguments may be brought. The one most relevant (from the standpoint here adopted) is that the concept of the "realities" which are to replace the sensible *data* are themselves abstracted from those data. Thus Duhem* not only argues, in a spirit entirely consonant with the spirit of this essay, that water is *not* really the hydrogen and oxygen which disappear when it is formed, but also shows that the atomic hypothesis upon which it is possible to conceive the "elements" as still present in the "compounds" is derived historically from Newton's famous Query 31.† In this passage Newton suggests the application of the ideas that he had gained from his study of planetary bodies to the analysis of the behaviour of the bodies manipulated in experiments. Similar observations occur in several of Mr. Merz' splendid chapters, and have been repeatedly illustrated in the course of our discussions in Chapters III and IV. More recently still it has been pointed out‡ that the most thorough-going quasi-metaphysical attempt to account for perceived physical events is vitiated by the same circle. The most striking feature of the electric theory of matter is that it exhibits the property of "mass" as the consequence of the motion of "electrons." But to reach this result properties of the electromagnetic field are appealed to, and these properties are defined by differential equations into which the notion of mass derived from the study of molar bodies itself enters.

* In his *Le Mixte et la Combinaison chimique*, 1902, and in other writings. See also *supra*, p. 82.

† *Supra*, p. 78.

‡ *See* a review of works on "Electrontheorie," by H. A. Wilson, in *Nature* for June 22, 1905.

§ 65.

Opposed to the thinkers who adopt the view of the value of scientific concepts which has just been repudiated, are those who have felt themselves forced to take up one of the various positions included under the name of the *descriptive* view of Science. Most of these positions have a relation to the wider philosophical position of Humanism,* which makes them particularly interesting at the present moment.

"The great Poincaré," says Professor James,† misses Humanism by a hair. He has demonstrated‡ in a brilliant manner the conventional character of Science, and has laid special stress upon the manner in which one theory has succeeded another in the same physical field.§ He appears to accept what we may perhaps call the disintegrating results of mathematical physics, regarding perceived things and events as really due to the superposition of a great number of similar elementary phenomena.|| Moreover, he removes from the Objective every element—such as the secondary qualities—which cannot be proved to be "the same for all" by the use of language. "Pas de discours, pas d'objectivité."¶ If, then, perception gives us no reality and the hypotheses of Science are only conventions, what is there that remains? We find that while hypothesis may succeed hypothesis—as, for example, Maxwell's electro-magnetic theory of light succeeded Fresnel's undulatory theory—the differential equations remain the same, the expression of veritable relations between real terms which Nature hides from us eternally, though Fresnel may think of them as *movements* and Maxwell as *electric currents*.** It is

* *See* James, "Humanism and Truth," *Mind*, N.S., No. 52, p. 462.

† James, *loc. cit.*

‡ In the essays reprinted in *La Science et l'Hypothèse*, and the more recent *La Valeur de la Science*.

§ See, e.g., *La Science et l'Hypothèse*, Ch. X.

|| *Op. cit.*, p. 187.

¶ *La Valeur de la Science*, p. 262.

** *La Science et l'Hypothèse*, p. 190.

through its knowledge of these Objective relations that Science has so much theoretical hold over the inscrutable reals, that it is able to predict the future; but that same knowledge has clearly a certain "intellectual" value quite apart from its value as a collection of *recettes pratiques.*

§ 66.

Even this amount of intellectual value seems to disappear in the view of Science advocated by M. Le Roy.* For this writer the laws of Science, when they are not conventional definitions, are simply *recettes pratiques,* "not *true* but *efficacious,*" "not concerning our *knowledge* so much as our *actions,*" "rather enabling us to *capture* the order of Nature, than *revealing* it to us."† Moreover, these laws have reference to artificial facts—*faits scientifiques*—created by the scientist out of the *faits bruts* of perception.

M. Le Roy's scientific fact seems to correspond to a large extent with our "secondary construction" by which the "primary fact" is apperceived. An "atom" and an "eclipse" are examples given. Poincaré adds an "electric current" as the scientific fact constructed from the brute fact of a galvanometer deflection; also the "corrected reading" obtained by treatment of a number of direct readings. We may add ourselves the "rigid bar" by which the actual elastic lever is replaced in theory. But there is this important difference between Le Roy's conception and our own: the laws of science as conceived by him seem hardly to touch the brute facts, which, not being scientific, are outside Science.‡ This is why the law is in so many cases merely a rule of action. In our view, on the other

* *See* the discussion reported at length in the *Bulletin de la Société française de Philosophie,* Mai, 1901. Le Roy's views are criticised by Poincaré in the essay reprinted in *La Valeur de la Science,* Ch. X.

† *Bulletin,* p. 5.

‡ Poincaré, *La Valeur de la Science,* p. 221. Cf. *Bulletin,* p. 21, where M. Le Roy says, "C'est ce qu'on ajoute au fait brut pour constituer le fait scientifique qui est le plus important.'

hand, the whole object of the secondary construction is to render the primary facts intelligible, to bring out real relations between the brute facts which constitute the scientific fact, and to lead to the discovery of new brute facts related to those already recognised within the system.

The same kind of inversion of the relations of primary fact and scientific construction is shown by the illustrations given of the *dictum* that laws are frequently definitions. Such a one is the law that "phosphorus melts at 44°," which is asserted by M. Le Roy to be merely a definition of phosphorus. One feels here in a peculiarly tantalising form the want of security of the relations between ideas and the reality beyond which some of us find in other presentments of Pragmatism. The definition "works," substances melting at 44° are actually encountered, but one has about their identity much the same kind of doubt as pursued the school-boy who feared that Shakespeare's plays were not written by Shakespeare but by another man of the same name.

§ 67.

Mach's splendid labours in this field are too well-known to need characterisation. For the founder and chief apostle of the new doctrine the concepts of Science are, as with us, means to an end, an end which is conceived as "the economic exposition of actual facts."* It is clear that this principle of "economy" pushes analysis further than the principle of intelligibility which we have been considering. It suggests, as Mach† applies it, a value for the race as well as for the individual in what we have thought of simply as a psychological phenomenon. This suggestion is of the highest interest and importance, and as such may be gladly accepted. But when the same circumstance is made the ground upon which Mach is claimed by Professor James (in the article already quoted) as a Humanist,

* Mach, *Science of Mechanics*, 1902 ed., p. 555.

† See *op. cit.*, pp. 481 *et seq.*

it seems necessary to determine what are the exact admissions implied by one's applause. I am prepared to admit that the results of Science *have* this economical value; prepared to admit that by Natural Selection or in some other way Nature may have arranged that Science shall be pursued so that this value shall be secured to the race; but, as before, I hesitate when asked to grant that this relevance to purpose constitutes the *essence* of the results in question. And Humanism is nothing more than an interesting genetic psychology if we do not take it as telling us not merely the circumstances under which we come to *recognise* such things as thinghood, or the conservation of energy, but what they are prior to our recognition. My own view of the principle of the conservation of energy I have endeavoured to explain. It is a concept by means of which a definite range of given facts is made intelligible to an individual thinker. In consequence of this circumstance it has an economical value. Further, it is the property of "secondary constructions," into which such concepts and the corresponding primary facts enter, that they lead to the "apperception" of new primary facts—reals or relations between reals—this being the external characteristic which distinguishes the scientific from other attempts to render the primary facts intelligible. Finally, the conception is a convention in that another could conceivably have been found to render the same facts intelligible, and, if "scientific," would have led to the recognition of the same real relations between the real things. The conception, in fact, plays the part which Lotze attributes to all ideas—the part of a tool which fits the mind and also fits reality.

If pressed to consider also the case of thinghood, I should have first to remark that I find between concepts of this order and the concepts of Science a distinct break. In this I differ from Mach, who does not appear to distinguish the process by which we supply a core to a mass of sensations, and so create a "thing" from the process by which we make a secondary

construction out of certain *data* by means of the concept of a transference of something (" energy ") that remains constant in amount. We seem to have here the thought which Professor James expresses in his article on *Humanism and Truth,** and the writers of the essay on *The Nature of the Hypothesis.*† According to this thought Reality is not the same after our judgment as before; it is "increased and elevated" by the act of judgment. The implication seems to be that scientific judgments simply continue a process which " common-sense " judgments begin. There are aspects of the two processes of judgment of which this notion of continuity holds good; we may grant to Messrs. Ashley and Dewey that the hypothesis *is* a predicate, and to Mach and Professor James that the concepts, both of "thing" and "energy," are economical. But, as I have already pointed out, " the secondary constructions" of Science which correspond to the "reality qualified by an ideal content" of the ordinary judgment contain no element that is not drawn from the common-sense stratum of consciousness. For example, if one body is cooling while another is simultaneously growing warmer, the secondary construction in which these primary facts are synthesised contains besides these facts merely the thought of another *thing* being transferred from one body to the other. On the other hand, the synthesis by which we bind the various qualities into the "thing" does not present us with anything analogous to this. The secondary construction is of a totally different character from the elements; the process does not reach its end by the ideal addition of a new element of the same type. Further, the hypothesis has, we have shown, merely a transient function. Setting aside purposes of exposition and convenience in conceptual handling, its function is to point the way to the discovery of new facts, including relations, and then to

* P. 468.

† In *Studies in Logical Theory*, ed. Dewey, 1903.

efface itself. Finally, at any moment it is at least ideally possible by criticism of the whole construction to separate the primary facts from the interpretative "embroidery," and to realise that the synthesis was not strictly inevitable. Whewell's *dictum* that "fact and theory have no essential difference except in the degree of their certainty and familiarity. Theory, when it becomes firmly established and steadily lodged in the mind, becomes fact" *—which is approved by Professor Dewey†—ignores this power of critical analysis. It is, in fact, an early pragmatic pronouncement which, like most pragmatist doctrines, is excellently true within its proper sphere of application. From the standpoint of the practical man—the scientific workman in the engineering workshop, the laboratory, or the study—the theories of his predecessors may often and may well be his facts. No one would censure a Marconi for accepting the Maxwell-Hertz theory of electro-magnetic ether waves as the factual basis of an attempt to bring England and America into wireless telegraphic communication; or a German chemist for directing his search for the means of manufacturing a "natural" perfume in accordance with the stereo-chemical concept of the molecule introduced by Van t'Hoff, or the "ring" concept of Kekulé. But the circumstance that theories are often taken with more or less naïve conviction as the primary *data* for sequences of actions in practical life, does not contradict the doctrine, the truth of which is established by such critical investigations as those of Mach, that *all* theories are ultimately founded upon a solid primary basis of Objective fact, which can, in most cases, be exposed with certainty below the superstructure which ages of man's toil and genius have erected upon it.

---

* Whewell, *The Philosophy of the Inductive Sciences*, 1840, p. 45.
† *Op. cit.* p. 164.

§ 68.

The admission that an actual break of continuity occurs between the ordinary judgments of common-sense and the judgments which involve hypotheses belonging to the class which we distinguished in § 63 will probably be obtained without difficulty. A less ready acquiescence may be expected when we proceed to maintain that a break can also be distinguished between common-sense judgments and scientific judgments which involve hypotheses of our first class. It will be remembered that these hypotheses consist in the interpolation or extrapolation of facts homogeneous with the *data* so as to form spatio-temporal systems of primary fact consonant with experience. The very circumstance that the added elements present this homogeneity with the *data* makes the separation between the common-sense judgment and the scientific judgment on this plane less conspicuous than in the former case; nevertheless, it may be held to exist.

A trivial example may make the distinction clear. If I say, "that man has a rolling gait," the synthesis has the inevitable character that is the mark of the primary fact, the common-sense judgment. If, on the other hand, I assert (on the ground of his rolling gait) "that man is a sailor," my synthesis has the secondary character which is not inevitable. If then you ask me if such a judgment is "scientific," I do not think I ought to hesitate to say "Yes" simply because the instance is trivial. The secondary construction is undoubtedly a reaction upon certain primary facts, and it has the property of leading to the observation of other primary "substantive" facts, and yet other facts, relations between these. In these respects it seems precisely like such a judgment as "this substance is copper sulphate," based upon an experiment in chemical analysis—a judgment which would generally be admitted as scientific. The former judgment, in fact, is related to the "unconditional universal," "All men

with such and such a rolling gait are sailors," in the same way as the latter is, to "all things that have such and such properties are copper sulphate." Both these assert a "permanent connection of qualities in the Real"—that is, are the final products of a process in which primary facts have been unified, systematised, or made intelligible by a concept which has not failed to lead to discoveries of fresh primary facts without limit in its province.

To this argument one very obvious answer suggests itself. If the judgment "that man is a sailor" is to be described as a secondary construction upon a basis of primary or presented fact, because in the mind of the observer a synthesis of this basis with non-presented elements occurs, how can the same term be withheld from the judgment "that man has a rolling gait"? Surely (it may be maintained) the application to a certain complex of presented elements of the term *man* implies the erection on the basis of that complex of a secondary construction into which a multitude of non-presented elements enters. The idea *man* is itself, in fact, nothing but an hypothesis whose function is to render the primary *data* intelligible; for it contains a reference to an immense body of spatio-temporal experiences homogeneous with the given material.

I venture to doubt whether this argument is really as strong as it is generally assumed to be. Granting that, as modern theories of perception teach, the primary *data* are "complicated" by references to non-presented material, and that the secondary construction gives the presented object its meaning, it seems possible, yet, to maintain that in the common-sense judgment the essential feature is an act of analysis or discrimination, and that the secondary non-presented elements are confined in the main to the function of supplying a motive for this discrimination. On this view the term "man," when it appears in a common-sense judgment, connotes merely a number of presented facts—very largely

visual facts. If Madame Tussaud can frame of wax and paint so cunning a counterfeit that the country cousin commits himself to the judgment "there is a policeman" and proceeds to act upon it, we have a case in which complication has followed with, perhaps, excusable haste upon an imperfect discrimination of the *data*. When the counterfeit fails to react to a question after the manner of genuine policemen, more careful discrimination at once takes place and reveals the absence of some essential visual elements of the complex to which the name "man" is assigned, or the presence of others which are incompatible with that complex.

This account must be qualified by the admission that very often it is impossible to tell from the form of the judgment whether it is of the common-sense type or the scientific. Hearing a certain peculiar cough and a certain shuffling step in the hall I flee incontinently from X, whose long-winded stories are the bane of the club. Here my action may follow upon a scientific judgment. The complication, instead of serving as the motive for a more careful inspection of the *data*, leads to the formation and acceptance of a secondary construction based upon those *data*. It is, on the whole, more "economical" to act upon this construction than to wait until so much of the Objective complex is presented as will render discrimination on the common-sense level possible. Thus while in a given case the judgment which follows upon the presentation of the given primary facts may belong to either of the two types distinguished, and the *occasions* upon which the reaction upon the presentations takes the one form or the other are not separated by any clear intrinsic difference, yet the judgments themselves are so essentially different that we are justified in denying to them the continuity which the pragmatic theory of knowledge seems to demand. And the difference for which we have found it necessary to contend at the level of scientific hypotheses of our first class is so plainly recognisable at the level of hypotheses of the third class that

to establish it needs no arguments beyond those that have already been considered in previous sections.

§ 69.

The doctrine of the primary facts presented in this essay is a variety of what has recently been called the "New Realism";* and I hesitate to say that I have derived it from Mr. G. E. Moore and Mr. Bertrand Russell only because I have no right to suggest the responsibility of these philosophers for details which they might quite possibly reject. The essence of the doctrine is the view that a large part of the contents of our consciousness from moment to moment consists of elements which exhibit themselves as having a certain unique "priority" to our conscious processes. These elements constitute what I have described as the Objective. They fall into three well marked genera—physical existents, psychical existents, and subsistents, which share with the former the characteristics of being regarded as "the same for all," and of having a certain relevance to human purpose, expressed by saying that "they have to be reckoned with."

In our consideration of the first of these genera we found ourselves driven to defend in substance, but to modify in detail the common-sense notion which regards the material world as composed of Objective "things" into whose structure primary and secondary qualities enter on equal terms; occupying at different moments of an Objective flux of time definite points in an Objective space. We also devoted some attention to the fact that the elements of the Objective are very often members of series, and gave special study to the series of numbers which plays so important a part in the development of Science.

The aim of the scientific process as it occurs in the individual is to render the Objective in its actual determinations intelligible. This happens when primary facts enter into

* *See* an article by Professor J. S. Mackenzie in *Mind*, N.S., No. 59.

an "apperceptive system." They may be apperceived by means of any concept drawn from any other context of experience, and if by means of this concept the actual particulars of experience are systematised, the "end" of the process will have been reached. But if the process has been of the kind intended by the term scientific, it will have the further property of leading to other determinations of the Objective, and these further determinations are the actual achievements of Science, and its "end," therefore, from the universal point of view. Since primary facts present themselves for the most part in series, the most useful method of determining the Objective consists in correlating terms of these series with the members of the number series—the property of this series being that single members of it can be substituted for combinations of other members in accordance with definite laws easily applied. By means of such combinations it is often possible to characterise simply the relations between things, and to ascertain what changes in terms of relations can be regarded as complete expressions of those relations. Such cases typify the ideal of the scientific process which is actually exhibited in a large number of grades, which nevertheless are sharply distinguished from the processes by which the Objective itself is recognised—a fact which is claimed in support of the view of the unique character of the latter.

In virtue of this feature our doctrine presents a contrast with the pragmatist theories of Science which regard the primary facts and secondary constructions as merely different stages of a homogeneous productive process to which it is impossible to assign either a lower or an upper limit.

Finally, it may be claimed that the concept here defended avoids the error contained in the theories of Science given by other writers, which have been justly criticised because they fail to represent the actual relations between hypothesis and fact. My concept allows the hypothesis

to determine largely what primary facts shall be apperceived, and admits that the fact before the individual, *i.e.*, the secondary construction, is constituted by the apperception. At the same time the implication that the Objective in this construction is an ideal upon which we can never actually set the finger, is rejected; and it is maintained that to a critical scrutiny the Objective reveals itself in ordinary cases, though in some cases it may not be easy to determine it without reference to the "confirmatory tests" of sameness for all and relevance to purpose.

HARRISON AND SONS, Printers in Ordinary to His Majesty, St. Martin's Lane.